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THE PSYCHOLOGICAL REVIEW.

ON SELECTIVE THINKING.[1]

BY PROFESSOR J. MARK BALDWIN.
Princeton University.

In a recent publication[2] I have used the phrase 'selective thinking' in a certain broad sense, and at the same time arrived at a view of the mechanism of the process which seems in a measure in line with the requirements both of psychology and biology. By 'selective thinking' I understand *the determination of the stream of thought*, considered as having a trend or direction of movement, both in the individual's mental history and also in the development of mind and knowledge in the world. The considerations suggested in the work mentioned are necessarily very schematic and undeveloped, and I wish in this address to carry them out somewhat further.

Looking at the question from a point of view analogous to that of the biologists, when they consider the problem of 'determination' in organic evolution, we are led to the following rough but serviceable division of the topics involved—a division which my discussion will follow—namely: 1. The material of selective thinking (the supply of 'thought-variations')[3]; 2. The function of selection (how certain variations are singled out for

[1] President's Address, American Psychological Association, Cornell Meeting, December, 1897. The paper aims to present rather a point of view, and to indicate some of the outstanding requirements of a theory, than to defend any hard and fast conclusions.

[2] *Social and Ethical Interpretations in Mental Development*, Macmillans, 1897.

[3] Wherever the word 'variation' occurs, the full term 'thought-variation' should be understood; this is necessary in order to avoid confusion with the congenital 'variations' of biology.

survival); 3. The criteria of selection (what variations are singled out for survival); 4. Certain resulting interpretations.

I. The Material of Selective Thinking.

I suppose that every one will admit that the growth of the mind depends upon the constant reception of new materials—materials which do not repeat former experiences simply, but constitute in some sense 'variations' upon them. This is so uniform an assumption and so constant a fact that it is not necessary to enlarge upon it, at least so far as the growth of our empirical systems of knowledge is concerned. But besides the constantly enlarging and varying actual experience of the world of persons and things, we have in the reproductive functions, taken as a whole, a theatre in which seeming novelties of various sorts are constantly disporting themselves. Seeing further that it is the function of memory, strictly defined, to be true to the past, to have for its ideal the reproduction of experience without variation, it would seem to be to the more capricious exercise of the reproductive function which usually goes by the term 'imagination,' that we are to look for those variations in our thought contents which are not immediately forced upon us by the concrete events of the real world.

A closer approach may be made, however, to the actual sources of supply of variations in our thought contents, by taking a bird's-eye view of the progress of thought looked at retrospectively; somewhat as the palæontologist puts his fossils in rows and so discovers the more or less consistent trend shown by this line of evolution or by that. When we come to do this, we find, indeed, certain consistent lines taken by our thought systems in their forward movement; lines which characterize certain more prominent series of stages in the descent or development of the mental life. First, we find the line of knowledges which reveal necessary fact, as we may call it; the line of correspondence between internal relations and external relations upon which Mr. Spencer enlarges, and to which the life of perception and memory must conform. Here there seems to be the minimum of personal selection, because all the data stand on approximatively the same footing, and the progress of knowl-

edge becomes mainly the recognition of reality as it is. Then, second, there is the line of development which shows the sort of concatenation in its members which goes in formal logic by the term consistency and results in some organization. This is often described as the sphere of 'truth' and belief, and is in so far contrasted with that of immediate fact. Third, there is the line of development whose terms show what has been and may be called 'fitness'—a certain very peculiar and progressive series of selections which go to build up the so-called 'ideals,' as in æsthetic and ethical experience.

In addition to these more or less selectively 'determined' lines of orderly arranged materials, there are of course multiple scattered products in the mind at all its levels; and these become especially noticeable when we cast an eye upon the outcome of imagination. We have in so-called 'passive imagination' or 'fancy,' in dreams, in revery, in our air-castle building, untold variations, combinations and recombinations. The question which comes up for answer in this first survey of these things is this: Do the variations by which the lines of consistent, or determined, development are furthered and enriched occur as accidental but happy hits in these overproduced *disjecta membra* of the reproductive processes?

I put the question at once in this way in order to come to close quarters with a current way of looking at selective thinking—indeed about the only current way. To be sure this question of selection has not been much discussed; but those who have concerned themselves with it have generally been content to say that in imagination, broadly understood, we have the platform on which the true, the good, the valuable thought-variations occur, and from the multitudinous overplus of whose output they are selected.[1]

[1] This seems to be the assumption, for example, of James (*Princ. of Psych.*, II. Chap. XXVIII). So also Dr. G. Simmel in an article (*Arch. f. Sys. Philos.* I., pp. 34 ff) which has come to my notice just as this paper goes to print; at least I find no suggestion in his article of any selection except that by movement, to which all thought-variations are alike brought through what he calls their 'dynamic aspect.' I may add that I find the general positions of Simmel on the origin and meaning of 'truth' so closely in accord with certain of the conclusions of my address that I regret the necessity of merely referring to them in the footnotes to certain of the following pages.

This, however, as it seems to me, is quite mistaken. We do not find ourselves acquiring knowledge in our dreams, thinking true in our revery, building up our æsthetic and ethical ideals through castle-building. We do not scatter our thoughts as widely as possible in order to increase the chances of getting a true one; on the contrary, we call the man who produces the most thought-variations a 'scatter-brain,' and expect nothing inventive from him. We do not look to the chance book, to the babbling conversation of society, or to the vagaries of our own less strenuous moods, for the influence which—to readapt the words of Mr. Stout—'gives to one of our apperceptive systems a new determination.' On the contrary, we succeed in thinking well by thinking hard; we get the valuable thought-variations by concentrating attention upon the body of related knowledge which we already have; we discover new relations among the data of experience by running over and over the links and couplings of the apperceptive systems with which our minds are already filled; and our best preparation for effective progress in this line or in that comes by occupying our minds with all the riches of the world's information just upon the specific topics of our interest.

All this would lead us to a first negative position; a position which discards the view that the material of selective thinking is found among the richly varied but chaotic and indeterminate creatures of the reproductive faculty. Yet it would leave the positive answer to the question of the source of fruitful thought-variations still unanswered.

There are two alternatives still open after the view just mentioned has been discarded; one holding that it is the function of the mind to do its own determining, to think its own true thoughts, to discover the relations which are true, to bring its own forms, schemata, arrangements of parts to the manifold of sense and imagination, and so to construct its systems of knowledge by the rules of its own inventive power. This theory, it is plain, is analogous to the theory of vitalism, with a self-directing impulse, in biology; and it comes up also rather as an answer to the question as to the forms and catagories of mental determination than to that as to the material. For even though the mind have its

'synthetic judgments *à priori*,' as we may say in the phraseology of Kantian philosophy, still the question arises both as to the sources and as to the criteria—the local, temporal and logical signs—of the empirical data which are utilized in the forms of knowledge. I do not know that any one would be disposed to say that our knowledge of the external world, of the characters of persons, of the truths of history and natural science, are not attained through experience bit by bit; and the question to which the *à priori* theory gives no answer is: How are these bits found out? Even given the 'catagories,' what sorts of experiences fit the catagories, and how is the fitting done?

Leaving for a later section, therefore, the question of the origin of the categories and reverting to the only remaining real alternative, the first thing to be said is that two limitations confine us in finding the source of the variations which are available for the determination of our thinking, whatever the sphere or line of progress be. First, the new thought-variations, to be candidates for selection, are not mere stray products of fancy; but second, they are yet not outside the problem of selection from variations which arise somehow in the experience of the individual thinker. Having these two limitations full in mind, we find the third alternative—which in my own opinion all the facts go to support—to be this: *The thought-variations by the supply of which selective thinking proceeds occur in the processes at the level of organization which the system in question has already reached—a level which is thus the platform for further determinations in the same system.*

Having stated this general position, we might examine each of the lines or spheres of selective thinking already pointed out; but that does not seem to be necessary. It is just the evident difference between the child and the man, say, that the former proceeds to test data which the latter never thinks of testing. The child thinks the moon may be made of green cheese, that birds may grow on the limbs of trees, that the sun does set around the corner of the world, that eating bread-crusts does make the hair curly; such conceits the man smiles at. The difference is that at the child's level of what we go on to call 'systematic determination,' these are variations of possible value; he has yet to

test them; but to the man they are not on the level or platform which his selective thinking has reached; they are not in any sense candidates for selection; they do not even enter into the complexly distributed series of thought-variations within the limits of which his criteria of value and truth lie. Various reasons have been given for this in the literature, and however they differ as explaining principles they are yet severally available as against the theory that all our imaginings afford a chance —and the more, the better the chance of profit. The untruth of this is what concerns us.

In getting his information about nature, the child learns by experimenting, as also do the animals. But having learned this or that, *he proceeds on this or that to learn more.* In judging a statement he scouts *in advance* what his lessons have already discredited. In admiring the æsthetic and in adhering to the good, he hesitates only where his sense of worth does not positively go out; what is to him ugly and bad he repudiates with emphasis.

We might take up the parable on the side of brain processes and ask what brain variations give good, true, fit, conscious states; and the same would be seen. Suppose, for example, that sane intelligent thinking over the data of the knowledge which one has already acquired involves some sort of coördination of the sensory and motor areas. This coördination is a matter of growth by integration. Variations to be fruitful — whatever be the tests of survival — must be variations in the functioning of this system. Suppose the visual center rebel and lose its coördination with the motor, or suppose the hearing center fail of its blood supply, and so drop away from the system; such changes would be gross accidents, temporary inhibitions or diseases, not variations to be selected for the upbuilding and enriching of the system. To be this, brain changes would have to take place in the delicately adjusted processes which constitute the essential coördination in question. I take this case, because, as will appear later, it suggests what is to my mind the real mechanism of thought — coördination of data in the attention, a motor function.

So far it has seemed that in each case thought-variations

must be all at a certain level if any of them are to be available for selection at that level. We may go a step further in the way of defining what is meant by 'level.'

It is just of the nature of knowledge to be an organization, a structure, a system. There is no such thing as mere 'acquaintance with' any thing; there is always—to abuse James' antithesis!—more or less 'knowledge-about.' And the growth ofthought is the enlargement of the 'knowledge-about' by the union of partial with partial 'knowledges-about' in a constantly wider and fuller system of thoughts. Selective thinking is the gradual enlargement of the system, a heaping-up of the structure. If this be true, a little reflection convinces us that variations in the items of material merely, in the stones of the structure, in the brute experiences of sense or memory, can not be fruitful or the reverse for the system. It is variations only *in the organization* which can be that. It is the readjustments, the modifications or variations in the 'knowledge-about,' which constitute the gain or loss to thought. A thousand flashing colors may pass before my eyes, a thousand brute sounds make a din in my ears, a thousand personal situations flit through my imagination, a thousand reports reach me through the 'yellow journals' of the condition of Cuba; but having no tendency or force to work changes in my organized systems of knowledge, they are not even possible candidates for my selection. The rich data of the world and of history might shower upon us; the music of the spheres might tickle our ears; the ideals of the Almighty might be displayed before us in color, form and action; but, be we incapable of organizing them, they are 'as sounding brass and a tinkling cymbal.' The things of time and eternity may vary infinitely in their appeals to us, but unless *we vary to meet them* they can not become ours. So do we find actually fruitless and barren, not only the kaleidoscopic changes, the variations on variations, of our dreams and our fancy, but equally so the pages of mathematical symbols in which we have not been trained, though they embody the highest thoughts of some great genius. They do not fit into the coördinations of knowledge which are ours, nor bring about readjustments in the arrangements of them. The items, to appeal to me, must never

quite break with the past of my knowledge; each must have its hand linked with that of the thought which begot it; it must have a 'fringe' if it is to get a lodgment upon the strings of my intellectual loom and stand a chance of being woven into the texture of the carpet which is to cover the upper floor of my mental residence. The burden of mental progress, then, seems to me to lie on the side of the organizing function.

We may believe, therefore, so far as we have gone, that the material available for selective thinking is only of the sort which reflects rearrangements, new adjustments—in short, new 'determinations'—in our organized systems of knowledge; and further that each of such candidates for selection is born, so to speak, at the top of the cone, at the highest floor or level, of its own peculiar system. Other fragments of thought, *disjecta membra* of imagination, lie scattered about the bottom, unavailable and useless. With so much said about the material, we may now go on to consider the process or function of selective thinking.

II. The Process or Function of Mental Selection.

In the consideration of this problem—of course, the most important one—the advantages of employing the genetic method will become apparent; and it may be well to distinguish the different spheres of mental determination somewhat in the order of their original genetic appearance, the first sphere being that of our knowledge of the external world.

1. The function here is evidently one of an organization of the data of sensation in a way which shall reflect for our practical purposes, the actual state of things existing in the world. The selective process must be one which in some way concerns the active life, for it is only through the life of active muscular exertion that the appropriateness of revival processes can be tested. We have here again two alternative views which I have treated in detail in my book on 'Mental Development in the Child and the Race;' the one theory, called the 'Spencer-Bain theory,' teaching that all movements showing variation stand on the same footing, and that it is a matter of happy accident as to which of these turns out to be adaptive. Such movements so

found out are pleasurable; others, giving pain, are anti-adaptional. Through the mechanism of repetition on the one hand, and of inhibition on the other hand, the former are selected and so survive, and with them survive the feelings, thoughts, etc., which they accompany or secure. The other alternative—advocated in the work mentioned—holds that there is a difference in movements from the start, due to the conditions of waxing and waning vitality from which they spring; pleasure and pain attach respectively to these vital effects of stimulations, and so there is in each case of a selection of movements a platform or level of earlier vital adaptations from which the new variations are brought to their issue. This latter theory would seem in so far to get support from the fact brought out above, that such a platform of acquired adaptation—a level of 'systematic determination'—is present in all selective thinking. This view holds also that such adaptive movements it is which, by their *synergy* or union, give unity and organization to the mental life.

Apart from this, however, the two theories agree in making the selection a matter of motor accommodation.[1] The system of truths about the world is a system which it will do to act upon, both when we take it as a whole and when we go into its details.

Another thing follows, however—and follows more naturally from the second of the two theories mentioned than from the first—*i. e.*, that novelty, variety, detail of experience, can be organized in the mental life only in so far as it can be accommodated to by action; if this can not take place it must remain a brute and unmeaning shock, however oft-repeated the experience of it may be. It itself, considered as a thought-variation, as well as the variations in it, would be as if non-existent—altogether without significance for the individual's growth in knowledge. The seat of productive variations of variations, that is, from which selections are possible, must be on the motor

[1] I am not sure, however, whether Professor Bain does not here leave Mr. Spencer behind. The latter nowhere, to my knowledge, discusses selection in the sense of mental determination, but his insistence upon the direct action of the environment on an organism would seem to require him to hold that the stimulations compelled the organism to accommodate in this direction or that, the motor-selection simply coming in after the fact of determination.

side, in the active life.[1] Only thus could 'internal relations' be established which should be true to or should reproduce 'external relations.'

The point of contrast noted above between the two theories has, however, an additional interest in connection with our present topic: the point that on my theory there is a platform of earlier habitual adaptations from which the variations are always projected. For this transfers the first selective function from the environment to the organism, requires the new experience to run the gauntlet of habitual reactions or habits which organize and unify the system of knowledges, before it can be eligible for further testing by action. For example, a child can not play the piano, though he might actually go through a series of movements reproducing those of a skilled performer. The multitude of variations, so far from aiding him, is just the source of his confusion. But he can learn little by little, if he practice faithfully on the platform of the movements of the simple scales and finger exercises which he already knows how to perform.

The first test, therefore, is that of assimilation to the platform of habit. If we grant this, and also grant that subsequently to this there is a further selection, from such variations, of those which work in the environment, we get a double function of selection: *first, the sort of intra-organic selection called above 'systematic determination' which is a testing of the general character of a new experience as calling out the acquired motor habits of the organism;*[2] *and second, an extra-organic or environmental selection, which is a testing of the special concrete character of the experience as fitted, through the motor variations to which it gives rise, to bring about a new determination in the system in which it goes.*

These selective tests we may call respectively the test of 'habit' and the test of 'accommodation to fact' (the latter

[1] By 'motor' I mean vaso-motor and glandular as well as actual muscular experiences; all of these considered as giving a reflex body of organic contents which cluster up upon incoming stimulations from the external world. It is all afferent, kinæsthetic, in its actual mechanism.

[2] The phrase 'intra-organic selection' suggests (intentionally on my part, although used here in a purely descriptive sense in antithesis to extra-organic) the process of adaptation called 'Intra-selection' by Weismann and described earlier by Roux under the phrase 'Struggle of the Parts.'

abridged to the test of 'fact'). These two functions of selection work together. The tests of habit, the intra-organic tests, represent an organization or systematic determination of the things guaranteed by the tests of fact; and, on the other hand, things which are not assimilable to the life of habit can not come to be established as intelligible facts. The great difference between the two tests is that that of habit is less exacting; for after a datum has passed the gauntlet of habit—or several alternative data have together passed it—it still competes for survival in the domain of fact.

What, then, do we finally mean by *truth* in the sphere of external knowledge? This, I think: a truth in nature is just something selected by the test of fact (after having passed the guantlet of habit, of course), and then so passed back into the domain of habit that it forms part of that organization which shows the 'systematic determination' of the thinker. What the word 'truth' adds to the word 'fact' is only that a truth is a presentative datum of the intra-organic system which has stood the test of fact *and can stand it again*. A truth is an item of content which is expected, when issuing in movement, to 'work' under the exactions of fact. We speak of a correspondence between the idea and the fact as constituting truth; and so it does. But we should see that a truth is not selected because it is true; *it is true because it has been selected*, and that in both of two ways: first, by fulfilling habit, and second, by revealing fact. There is no question of truth until both these selective functions have been operative. This is to say, from the point of view of motor development, that accommodation always takes place from a platform of habit, and that in the case of the external world our first-hand knowledges arise as reflexes of such accommodations.[1]

[1] In my book (*Soc. and Eth. Inter.*, Sect. 57) these two phases are generalized as follows: "With the formula: *what we do is a function of what we think*, we have this other: *what we shall think is a function of what we have done.*" In general conception this is Simmel's position. In the following sentence (of which the passage in the text might almost be considered an English rendering) he is accounting for the 'Harmonie' between thought and action; he says: "Dies (Harmonie) wird erst dann begreiflich wenn die Nützlichkeit des Handelns als der primäre Faktor erscheint, der gewisse Handlungsweisen und mit ihnen die psychologischen Grundlagen ihrer züchtet, welche Grundlagen eben dann in theoretischen Hinsicht als das 'wahre' Erkennen gelten; so das ursprunglich

2. In the life of general and ideal thinking the same questions come upon us. Here we have, it is true, a certain restating of the problem, but it seems that in its essential features the principles already worked out have application. First, as to the platform—for as we saw above, thought-variations to be selected must be projected from a platform of earlier progressive thinking or systematic determination. The platform on the side of function—that is, apart from the content organized—is, I think, *the attention*. The attention is a function of organization, a function which grows with the growth of knowledge, reflects the state of knowledge, holds in its own integrity the system of data already organized in experience. I shall not dwell long upon this, seeing that it will be generally admitted, I think, that attention is in some way the organizing function of knowledge, and also because further definition—which, moreover, I have attempted elsewhere[1]—is not necessary to our present purpose.

The first selection which thought-variations have to undergo, therefore, if eligibility from this platform be the first condition of final adoption, is in their getting a place in the organization which present attention-conditions represent and exact. This is just the condition of things we saw above when we pointed out that it is only the strenuous, hard and attentive concentration of mind that brings results for the life of thought. Attention is relatively easy, when we let it roam over our old stock in trade; but even then the contrast is striking between the items of knowledge which are held in the system thus easily run through with frequent repetition, and on the other hand those vestigial fragments of representation which do not engage the attention in any system of exercises, and so have no settled place or orderly se-

das Erkennen nicht zuerst wahr und dann nützlich, sondern erst nützlich und dann wahr genannt wird" (*loc. cit.*, p. 43). Simmel makes the further argument that in animals of lower orders having senses different or differently developed from ours, the motor accommodations by which the sense organs have arisen must be to different forces and conditions in the environment. So what would be counted 'truth' in the mental systems of such creatures would vary among them and also from our 'truth' (*loc. cit.*, p. 41.) An important point of difference between Simmel's view and my own is noted below.

[1] *Mental Development in the Child and the Race*, Chap. XV., where it is held that the attention, organically considered, is a habitual motor reaction upon mental contents.

quence in our mental life. The latter are not *on the platform;* the former are. There is always such preliminary 'intra-organic' selection—a set of ready interests, preferences, familiarities, set to catch our new experiences or to reject them. It proceeds by motor synergy or assimilation. Thoughts which get so far in are then candidates for the other selection which the full term 'selective thinking' includes. In order to be really the thought-variations which selective thinking requires, all new items must, in the first place, secure and hold the attention; which means that they must already enter, however vaguely, into the complex of earlier knowledge, in order that the habitual motor reflex, which attention is, may be called out.

In considering in another place the empirical complex mental contents which constitute attention,[1] I found it necessary to distinguish three sets of motor events; and I threw them into a so-called 'attention formula,' as follows: *Att.* (attention) = $A+a+\alpha$; the A representing the gross and relatively constant reflex effects which give attention its main sensational content; the a representing the special contents which vary with different classes of experience, as for example those of the different sense-qualities; and the α representing the refined variations which attention to particular objects as such brings out. It is a part of the general analysis of attention which issues in this formula that the state of mind called 'recognition' varies as some or other of these elements of attention are present without variation through repeated experiences. All are present without variation when we recognize a particular object as familiar; there is variation in the α elements only when we are able to place a new object in a familiar class but yet do not find ourselves familiar with it for itself; there are variations in both the a and the α elements when a novel experience simply meets the general requirements of our grosser life of habit, but yet has no place in the organization of our knowledge,[2] a state of mind characterized by socalled 'reality-feeling.'

[1] *Mental Development in the Child and the Race*, Chap. X., § 3, and Chap. XI., § 2.

[2] Thus the animal instincts show gross motor reactions upon the objects which call them out, and it may be that the only differentiation of the objects possible to the creature is just that supplied by his differentiated instinctive attitudes.

This analysis enables us to see more clearly the meaning of the 'platform' from which thought-variations must be projected to be real candidates for selection in the life of attention. The experience which does not even bring out the constant *A* elements is merely a brute shock and not 'knowledge-about,' seeing that in these elements, which are necessary to all attention, we have just the gross motor adaptations in which accommodation to the external world consists. Such 'shocks' do not reach the platform.

Further, those experiences which do involve the *A* elements must also, at least in selective thinking, have some sort of *a* element and *a* elements with them; seeing that, in the realm of thought, attention which is not concrete involves no specific determination.[1] The study of the child shows that the differentiation of the *a* from the *a* elements is a gradual thing, the first knowledge being of a 'vaguely universal' sort (an expression of Royce's; the same thing has been called by the present writer 'the general of the first degree.') Psychologically, therefore, the platform upon which the new knowledge is to be secured is that of a sense of familiarity toward an experience, at least in the unrefined way which the child's 'vague universal' illustrates. The apprehension of a new truth is always either the consciousness of an identity, in which case it is treated as an old truth in all respects, or it is in some measure subsumed under an old truth, and so illustrates class-recognition. And it follows as to our platform that any new knowledge, to be selected and held as such, must be capable of the sort of subsumption which class-recognition is. This gives, in the sphere of general thought, the analogue of the assimilation to habit which we found necessary to the establishing of the platform of progressive determination in the case of knowledge of external objects. The two cases taken together, therefore, constitute the function of 'systematic determination.'

But this is not yet selective thinking. The selection of the

[1] We often have, however, the familiar fact that the concrete content on which attention is fixed is merely a *point d'appui*, or symbol, verbal or other, which, on the organic side, merely opens a channel for the discharge of the larger wholes or attitudes (the *a* elements) which general and class notions presuppose.

particular concrete datum is more; it is *an affair of the selection of variations in the attention complex*, after the datum has passed muster in the systematic determination. It is an affair of the variations of the *a* sort, at the crest, so to speak, of the attention movement. How, then, are these selected?

I think, by a precisely analogous process to that which holds for muscular accommodation by adaptation to the environment; —*i. e. of*—it is a case of 'functional selection from over-produced movements.' It is here, as there, the environment's turn to get in its work, after the organism has had its turn. Yet here, as there, we must be careful to have a clear understanding of what the environment is.

The environment is here *the whole of knowledge not possessed by the individual thinker;* that is, the whole of the social store of opinions, beliefs, reflections, judgments, criticisms, etc., within which the individual displays his reasonable activities. The selection of thoughts as valid is analogous of the selection of facts as true. Apart from the direct necessity of accommodation and recognition which the physical enforces upon us, and which constitutes the selection of certain facts from all those possible but pseudo-facts which our habitual reactions might allow to pass—apart from such physical facts, all truths are selected by a testing in the social environment, from the many pseudo-truths which have passed the gauntlet of our habitual attention reactions.

To illustrate: We see a vague outline in the dusk; it might be a man, a beast, a tree-stump, as far as our present adaptive attitudes and recognitions avail to define it. To decide which it is and so to select one alternative as true, we put it to the physical tests of nearer approach, touch, hearing, etc. Here we have first the platform, then the selection by further action. So in thinking: We hear, let us say, a report that a friend is dead; he may have died by accident, by poison, by fire, so far as our information goes. We find out the truth, however, by getting information from some one who knows. Here, again, is the platform with its alternatives (variations), and then the selection by a social appeal. In the case of a scientific invention the part which can be attested by an appeal to fact is so tested,

but the part which still remains hypothetical is so far liable to social confirmation that the inventor at least expects others to judge as he judges.

The use of the word 'judge' in the last sentence serves to suggest certain further considerations, which show the social appeal in operation, and, at the same time, give evidence that it is this appeal which constitutes the resource in selective thinking in the higher and more ideal spheres. These considerations may be presented under the third heading.

III. The Criteria in Selective Thinking.

By criteria here I mean not so much objective criteria—marks or characters of this or that experience—as criteria of survival, *i. e.*, the tests or qualifications which new items of experience must fulfil if they are to be given a permanent place in the organization of knowledge. This involves the question of objective criteria, to be sure; but we may be able to find some general qualification under which the special criteria of the different provinces of knowledge may be viewed. Our question may be put in the familiar terms of an analogous biological problem, if we ask: when a particular truth has been shown by selection to be such, *why was it found fit to survive?*

In answer to this question we may say at once, concerning knowledge of the external world, that the motor accommodations by which the selective process proceeds are, by the conditions of the environment, *of necessity* made in this direction or that. The reason a given movement is fit is because it actually reports fact. The dictum of the environment is: accommodate to *xyz* or die in the attempt! The facts are there; nature is what it is; the adaptations are such just because they are fit to report a state of facts. The environment in which the accommodations take place, and to which they constitute adaptations, is the control factor, and its facts are just the only reason that the selections are what they are. The criterion here, therefore, is simply the adaptive aspect of the movement, as reporting fact. It can be determined in each case only after the event; that is, after the selection has taken place.

But even in this lower sphere, where the exigencies of the

physical environment are the control-factor in the selective process, we find the further result that the preservation of the fact selected depends upon its having already been assimilated to the organized habits of the individual. As knowledge it becomes part of a system; it is added to the platform from which subsequent selections are made; and it thus carries forward the 'systematic determination' of thought. In this way *the organism gradually reproduces in its own platform of determination, the very criteria of selection at first enforced only by the environment.* We should expect to find in consciousness some general coloring due to the attitude which the systematic determination platform requires—an attitude of welcome, of hospitality, of endorsement, in short, of *belief*—toward those facts which have passed through the selective processes, have been added to the organization of knowledge, and have acquired the *cachet* of familiarity.

I need not stop to argue that it is right to apply the term 'belief' to this sense of the internal fitness of experiences after their selection; the implied converse proposition, *i. e.*, that belief is a motor or active attitude, has been ably argued by Bain, James and others. I have advocated it in my book on 'Feeling and Will.' But whatever we call it, there is the fact—and that is what I wish to emphasize under whatever name—that even in our knowledge of nature, the individual *gradually builds up internally* the criteria of selection; and as his experience extends ever more widely afield from the brute resistances, strains, and contacts with things, he becomes a more and more competent judge for himself of the value of variations in his thoughts. Here is what is essential in it all: The sense of values has grown up all along under the actual limitations of control from the imperative selective conditions of the environment, and if one make use of his criteria of selection beyond the teachings of his experience it is only by means of those general rules which are implicit in the systematic determination itself.[1]

[1] Such as the laws of identity (motor habit), consistency (motor assimilation), sufficient reason (accommodation to the item selected), etc. Cf. *Mental Development in the Child and the Race*, Chap. XI., § 1. By these I think it possible to account for the so-called 'analytical processes,' which have to deal with relationships inside the whole of systematic determination; on which see further below.

Turning now to the great platform of attention, we find an analogous state of things, and the analogy really turns out to be identity of process, thus providing a strong argument for the view that the social criterion of selection is here the true one.

In the first place, we have to recognize that *in all thinking whatsoever as such* — even in our thinking about the external world when viewed not as motor accommodation, but as a system of organized truths—*the environment is social.* For we may ask: What does environment mean? Does it not mean that set of conditions which runs continuously through the individual who is said to be in the environment? The physical environment is such because its conditions are those of motion, and the organism moves. The environment of thought can only be thoughts; only processes of thought can influence thoughts and be influenced by them. The sources from which spring items in the world of thought are ordinarily centers of thought—minds, either one's own or some one's else. So the environment must be the persons about the thinker. They constitute his environment; they give him conditions to react upon; *they are the control-factor in his higher selective thinking*, just as the world of things is the control-factor in his life of sense-perception. I know that it is through their life of action—mainly indeed, the speech-functions—that he realizes their thought, and it is through *his* life of action that he reacts upon their thought and exhibits his; but even in knowledge of the external world of signs, expressions, etc., we have to say that movement must be reduced to some form of thought in order to be organized in our knowledge. And as soon as we get out of the sphere of knowledge of the world of things, and ask how knowledge can proceed without the selective control of physical fact upon movement, we have to say that if selection is to have reference to any environment at all it must have reference to an environment of thinking. Apart from theory, however, the social life is as a matter of fact the environment of our thinking; in my recent book on 'Social Interpretations' I have cited much evidence to show that the child organizes his thoughts with constant reference to the control which the social environment enforces.

So we have found that each group of thought-variations, to be candidates for selection, must be projected from a platform of acquired knowledge, represented on the motor side by certain elements in the attention-complex which give the sense of familiarity, class-identity, general truth or vague universality. This is the platform of systematic determination through the attention. Now, why not stop here? Because when a new thing comes, this does not suffice to secure those more refined elements of the attention-complex which determines a new concrete fact. On the contrary, many alternative determinations, all of them answering the demands of the platform of vague generality, might be forthcoming and the mind might rest in any one of them. Note the child's long continued and fanciful speculations about the simplest events in the household. What must now be had is just the selective control of an environment in which such variations can be brought to a test; and to the child this is the environment supplied by the persons who know more than he does. To them he normally appeals, almost invariably accepts their decisions, and finds certain of his alternatives thus selected, by what is to him as direct an adaptation to fact as are the selections of his movements by accommodation to that other environment, the world of things. Every new piece of knowledge needs this confirmation just in so far as the systematic determination by which it is brought to the bar of selection leaves the concrete filling of the event indefinite; that is, in so far as various alternatives or variations might be brought into selective rivalry with it.

But then—and this is the vital fact in the growth of the individual—this selection by a social criterion *becomes personal to the learner through his renewed action.* The selected functions, with their knowledge contents *are added to the organization within, so that the 'systematic determination' of the future is influenced by the assimilation of each new selected element.* Thus the inner attitude which the individual brings to his experience undergoes gradual determination by the continued selective action of the social environment. He himself comes more and more to reflect the social judgment in his own systematic determination of knowledge; and there arises within him-

self a criterion of a private sort which is in essential harmony with the social demand, because genetically considered it reflects it. The individual becomes a law unto himself, exercises his private judgment, fights his own battles for truth, shows the virtue of independence and the vice of obstinacy. But he has learned to do it by the selective control of his social environment, *and in his judgment he has just a sense of this social outcome.*

In the work referred to I have dwelt at length upon the actual facts of this educative dependence of the individual upon social lessons. The aspect to be emphasized here is the selective aspect, *i. e.*, the truth that the internal criterion is, so far as it goes, always in fact the primary criterion in our thinking; but that in its origin the relation is quite the reverse; and, further, that the individual's judgment is liable all the time to the final selective revision of the social voice. This shows itself most markedly in those ideal states of mind in which the direct control of objective fact is lacking and where the private determination is more or less explicitly accompanied by a sense of 'publicity'—a sense that the public judgment is implicated with one's own in the approval or disapproval of this act or that. In our ethical judgments I think this ingredient is unmistakable.

It remains only to say again that in the state of mind called belief, mental endorsement, and in particular cases judgment, we have the actual outgoing of this systematic determination upon the details of experience. All judgments in experience are, I think, acts of systematic determination, acts of taking up an attitude, of erecting a platform from which new things, to be eligible for selection, must be viewed. The details of organization, thus gradually built up, show the relationships of our theoretical thought; and these relationships are valid since they show just the motor organization which has accrued to the attention-complex. The data of fact or objective truth are the items which have passed through the selective ordeals.[1]

[1] "So erzeugen sich für unser Denken, gemäss dem Nützlichkeitsprincip, gewisse Normen seines Verhaltens, durch welche überhaupt erst das zustande kommt was wir Wahrheit nennen, und die sich in abstracter Formulirung als die logischen Gesetze darstellen" (Simmel, *loc. cit.*, p. 45.) It is here that I may note the difference between Simmel's view and my own. He makes (so far as the

The general conclusions which the sketchy development so far made would suggest may be stated in summary form before we go on to note some further points of interpretation in the last remaining section. These conclusions are as follows: Selective thinking is the result of motor accommodation to the physical and social environment; this accommodation taking place in each case, as all motor accommodation does, from a platform of earlier 'systematic determination' or habit. In the sphere of the physical environment as such, the selection is from over-produced movements projected out from the platform of the habitual adaptations of the members brought into play; in the sphere of the social environment it consists in the accommodation of the attention, secured by the over-production of motor variations projected from the platform of the habitual attention complex. The presentations from which the selected motor variations issue are believed or called 'true,' while the organization which the motor complex gradually attains holds the data of knowledge in relations of theoretical and analytical 'validity.' In the case of physical selection the internal organization represented by systematic determination gradually serves to free the organism from direct dependence upon the control of the environment;[1] in the intellectual life this is even more true, the development of the individual's judgment growing more and more independent of social control as progress is made in the 'systematic determination' which organized knowledge exhibits.

This general sameness in the operation of selection in the two spheres is what we should expect if the method of motor ac-

undeveloped form of his article justified an interpretation) the function of movement that of giving 'truth' to thought-variations *already present*. The 'dynamic aspect' in its issue secures the selection of the ready-formed 'presentative aspect.' This I hold to be true (when supplemented by the 'systematic determination' of the variations on a platform) of presentative data, wholes, or facts as such. But there still remain the determining effects of the motor-selections themselves upon the systematic determination. The synergies, inhibitions, etc., of the new motor accommodations with old habits produce changes in the *organization or relationships of the data* and give rise to theoretical and analytical 'validity' in our knowledge, which differs (as Simmel himself points out, and as Urban has independently suggested) from the objective 'truth' of given data or 'wholes.'

[1] This is seen in psycho-genetic evolution in the rise of memory, thought, etc., considered as variations.

commodation be what I have described in 'Mental Development in the Child and the Race' as the imitative or 'circular' reaction. For it is just through reactions of this type, with the antithesis between pleasure and pain by which they are furthered and maintained, that motor accommodations are all the while passed over to the domain of habit—that is, are integrated in the system of 'intra-organic determinations.' Thus organized knowledge in all its development may be looked upon as due to the *synergies* of motor processes selected as accommodations to the world of things and persons.

IV. Some Fragmentary Interpretations.

In the way of showing certain general bearings of the position now taken—bearings which the limits of this address do not enable me to amplify in any detail—I may go on to indicate the points which follow. They are suggestions toward a broader union of psychological with biological and philosophical points of view.

1. It will be seen that the position now taken up preserves what may be called the 'utility' criterion of survival through the whole progress of knowledge. The acts of selection are never independent of control from experience, however adequate they may be *within* this control; for the internal or systematic determination, while always the platform of variation, is yet never the final agent of concrete selection. To be sure, the individual's judgment, his sense of reality and truth, becomes more independent or self-legislative, as we have seen; but this, when genetically considered, is both the outcome and the evidence of the control which the environment has all along exercised. Even though we assume certain innate norms of selection which the individual directly applies, still these norms must not only lead to workable systems of knowledge in the world of active experience; but they must in their origin themselves have been selected from variations, unless, indeed, we go back to a theory of special creation with preëstablished harmony.[1] But if we

[1] *Cf.* an article on *The Origin of a 'Thing' and its Nature*, Psych. Rev., II., 1895, p. 551 (printed also in *Princ. Contrib. to Psychology*, I., No. 3, p. 105) for some criticisms of this theory.

admit that they are themselves selected variations, then we find no way to account for their selection except that by accommodation to the physical and social environments.[1] This preserves the utility criterion, therefore, even though we may not accept the precise method of selection portrayed above.

2. This does not tend, however, to give support to Mr. Spencer in looking to 'race-experience' for the origin of the categories of knowledge. Spencer's theory has been admirably criticised by Professor James, who thinks that the forms of knowledge must be looked upon as variations, not as accumulations from the repeated impressions of the environment. In support of James's argument we may add, what to me seems an insurmountable objection, that Spencer's position requires the transmission of such impressions by heredity[2]—a notion which James was one of the first to combat and a claim for which there is no evidence whatever. The position developed above assumes variations, with constant systematic determination in the individual's experience; in this the control of the environment is reflected. We then need a theory of evolution which will account for the determination of race-progress in the lines thus marked out by the individual.

3. This requirement is met, I believe, by the theory of Organic Selection, recently proposed by the present writer, considered as supplementary to Natural Selection in the way of securing lines of determinate evolution. According to this view, those individuals which successfully accommodate to the environment live and keep alive through heredity the congenital variations which they exhibit. To these are added further congenital variations which are again selected. Thus variations are secured in definite lines in a series of generations—lines which reproduce the determinations first secured in the individ-

[1] Simmel makes the analogous argument (*loc. cit.*, p. 45) that even if we had on *à priori* stock of knowledge, a selection of movements would still have to be made for practical life, and a system of 'truths' would be built up thereby.

[2] *Cf.* the dogmatic utterance of Wundt *apropos* of instinct: "The assumption of the inheritance of acquired dispositions or tendencies is inevitable, if there is to be any continuity of evolution at all. We may be in doubt as to the extent of this inheritance; we cannot question the fact" (*Hum. and An. Psychology*, p. 405). Hoc atque anno 1892!

ual under the control of the environment.[1] On this view, there would be a constant selection of individuals by natural selection, from a platform of organic selection which is analogous to the platform of 'systematic determination' in the individual. Race evolution would thus, on the whole, conform to the exigencies of experience, and would seem to be directly controlled by the environment, while due, in truth, to a series of variations accumulated by Organic and Natural Selection.[2]

4. Furthermore, the content of the intellectual and social environment is kept constant by the handing down of tradition through 'social heredity,' and the same demands are thus made upon the individuals of each new generation, both as to their concrete selections under the control of the environment and as to the forms in which the 'systematic determination' of knowledge is cast.

5. Finally, the 'systematic determination' of the individual thinker is, on the subjective side, just his *sense of self*. Judgment is the *personal* endorsement of the data of knowledge. Belief is a *personal* attitude. The person is the whole of the organization of knowledge; and as the social criteria of selection—and the social data selected—play an essential rôle in the process of systematic determination, as explained above, so the person is a social outcome. This I have developed at length elsewhere.[3]

[1] *Cf.* the summary in the PSYCHOLOGICAL REVIEW, IV., p. 393, article on 'Determinate Evolution,' where references are given especially to the writings of H. F. Osborn and C. Lloyd Morgan, who arrived at the same position independently. The theory of 'Organic Selection' has been indorsed by such psychologists as Wm. James and J. McK. Cattell and by such biologists as Alfred Russell Wallace and E. B. Poulton.

[2] Professor Poulton says, referring to Organic Selection: "These authorities justly claim that the power of the individual to play a certain part in the struggle for life may constantly give a definite trend and direction to evolution, and that although the results of purely individual response to external forces are not hereditary, yet indirectly they may result in the permanent addition of corresponding powers to the species. * * * The principles involved seem to constitute a substantial gain in the attempt to understand the motive forces by which the great process of organic evolution has been brought about."—*Science*, October 15, 1897, p. 585.

[3] In '*Social and Ethical Interpretations*.'

SOME EFFECTS OF SIZE ON JUDGMENTS OF WEIGHT.

BY DR. H. K. WOLFE.

Sight may well be called the universal sense. It contributes, perhaps, as few of the original elements of our knowledge as either touch or hearing, and in this regard it should not rank much superior to some of the other senses. On the other hand, its facility of association is so marked that psychologists of all times have been unable to agree upon the line separating 'native' and 'acquired' powers. Even in the new psychology there is little unanimity on the degree of 'voluminousness' possessed by sensations from the retina alone. One point, however, has been made remarkably clear by the newer investigations of the senses, viz.: the complexity of even the so-called simple perceptions.

A very crude analysis reveals the small part actually taken by retinal impressions in our knowledge of the external world. And yet our eyes are so actively engaged in the search for knowledge, that scarcely any phase or degree of the stimuli which act on other senses fails to produce also some effect on the retina. While this fact is especially noticeable in connection with touch and the muscular sense, it may also be observed in hearing, and even in case of the lower senses, taste and smell. Sight is indeed becoming so nearly a mirror of the senses that we often forget that its value depends largely upon its reflective power. Its fine shades of discrimination and its wonderfully tenacious associative activity enable it to become a factor in nearly all our perceptions, not only without effort but with a naturalness resembling that of instinct itself.

We are momentarily reminded of the versatility of sight. Its suggestibility seems unlimited. The taste of a strawberry, the smell of a rose, the feeling of a file, injuries and blood, the movements of musicians—in all these instances sight seems to

stimulate the appropriate sense organ, and to some extent it acts as a substitute sense. The degree of substitution, or indeed the existence of any such action at all, depends, of course, upon the previous action of the proper sense in connection with sight.

Whether the sense of sight is assuming the functions of the other senses to their detriment is, perhaps, uncertain. The relative importance of the senses is undoubtedly changing, and in this way sight is certainly distancing all others. Unless our power of acquiring knowledge is also increasing, it follows that the other senses must lose. It is not inconceivable that sight may yet develop much further, and that we shall succeed in breaking up the real or supposed circuit from the written word through *hearing* to the centers of speech and understanding. Why should we not obtain as clear an idea of the contents of a written page from one glance as of a picture? In a future paper I hope to consider some pedagogical questions illuminated by recent investigations along this line. At present we are concerned with one or two specific phases of the influence of sight on other sensations.

The basis for this article is a series of experiments made during the past four years, chiefly on advanced students of the University of Nebraska. The work was suggested by the well-known difficulty of judging the weight of light bodies in terms of pounds, or of heavy bodies. A pound of lead was known to be psychologically heavier than a pound of feathers; but I wish to know as nearly as possible in the time at my disposal, how much heavier, or how much feathers is as heavy as a pound of lead. This latter form of the question suggests the only method that can be employed with any degree of accuracy. It is, moreover, the only method recognized in experimental psychology for quantitative work.

I knew of no attempts along this line having been made or proposed, and, as is always to be expected in such cases, the first results were surprising and instructive.[1] Wood and lead

[1] In the summer of 1894, after my experiments for the first year had been made, Dr. F. B. Dresslar published in the *American Journal of Psychology*, Vol. VI., page 313 ff., an article on 'The Psychology of Touch,' in which this same problem was attacked in a different manner. Dr. Dresslar experimented by proxy chiefly on school children, who were requested to arrange in order of

were selected as the best materials at our command. Ten disks of wood three-quarters of an inch thick and varying regularly in diameter from two to twelve inches were prepared. These were well seasoned and were numbered in order from one to ten (I.–X.), beginning with the smallest. These ten disks served throughout all experiments hereinafter described.

The corresponding series of lead weights was not so soon determined. I first prepared fifteen lead disks, making the lightest about half as heavy as the smallest wood disk. Some preliminary experiments soon proved that lighter lead weights would be necessary. Three smaller weights were added, the lightest weighing a little less than one-fourth as much as the smallest wood disk. The experiments of the first year were made with these eighteen lead weights. It was found that the lead weights were too few. For the second year another series was made, beginning with one-seventh the weight of the smallest wood disk, and ending with number twenty-seven, which

weight a series of brass cylinders having the same diameter but varying in length by half inches, from one and a half inches to five inches. The cylinders were, in fact, of equal weight, but appeared unequal on account of the difference in size.

The next year Dr. Seashore published in *Studies From the Yale Psychological Laboratory*, Vol. III., a very interesting paper on 'Measurements of Illusions and Hallucinations in Normal Life,' in which he presents another method and its results. Dr. Dresslar did not emphasize the quantitative relations, since his method was not adapted to quantitative determinations. It is also a question whether the method employed by Dr. Seashore will yield true quantitative results. His standards were all of one size and differed in weight, while the companion series were all of one weight and differed in size. At first thought he would seem thus to avoid the double illusion. But when the smallest weight is compared with the lightest, or when the largest weight is compared with the heaviest, there is evidently a double illusion very unlike that obtained by comparing the medium *size* with the medium *weight*. Something of the nature of the illusion is also suggested by the conditions of the experiment. Suspicion is aroused by a series of cylinders uniform in size but varying in weight. With trained observers such as Dr. Seashore employed there is, of course, little objection on this account. For the untrained observer such as I have used, my own series seem to furnish the least objectionable method. It is the natural process and each person uses it almost daily. Before a quantitative relation between size and weight could be established, my method would require experiments on several substances. But it is doubtful whether such a relation can be established in any other way, so as to represent the normal, unconscious effect of size on our judgments of weight.

weighed slightly more than the largest wood disk. The same series was used also during the third and fourth years. It was impossible to prepare lead balls for this work and equally difficult to secure cylinders or disks of uniform diameter. Lead disks of four diameters were, therefore, used (½ in., 1 in., 1½ in., 2 in.). While this very considerable change in diameter introduces a cause of error which must be regretted and which will be referred to later on, it seemed to me the best solution practicable at the time.

The process of judging was the same every year. For this kind of work I do not believe it best to restrict the method of students. Constant method seems to be less important than constant subject. Each student lifted the weights as he pleased, with one hand or two hands, by edges, on tips of fingers, or in any way that suggested itself to the individual. Two to four students worked at the same time (on different parts of the series of course), and under circumstances which prevented any assistance whatever, except perhaps in the suggestion of variations in method of holding the weights. Either myself or an assistant was always present. Usually about twenty minutes were required for a series.

It would be interesting to have before us the separate judgments of each student, but with so large a number of individuals this is, perhaps, impracticable. I, therefore, have arranged the members of each class in alphabetical order, men and women in separate lists. Groups, *a*, *b*, *c*, etc., are then formed, consisting of approximately ten students each, in no case interrupting the alphabetical order. The tables contain the averages of each group for the various wood weights, together with the average variation of the individuals from the average of their group.

The results from the *individuals* of the first group of ten men, and also from the first group of ten women for the year 1896 are given in Table I. The figures at the head of the table show the weight of the wood disks, other numbers show the weight of the lead. These results are fairly characteristic of the entire company examined. They show the astonishing differences between the judgments of the sexes very well. They

do not happen to contain the extreme judgments, but they have nearly as large variations as the other groups, as may be observed in Table II. While a more or less regular progression from light to heavy is generally to be observed, this is sometimes seen to be inexplicably slow, as in numbers 5 and 10 of the women, and 8 and 9 of the men. The irregularity of number 9 of the men is an exceptional case.

TABLE I.

Showing individual judgments of men and women.

Wood.	15.5	35	61	87.5	126.5	174	231	295	364	525
Men.										
1	7.5	11.2	27.0	34.0	70.0	84	109	147	204	274
2	6.0	11.2	13.5	17.5	27.0	39	70	109	198	274
3	7.5	23.0	27.0	34.0	50.0	70	84	147	204	274
4	6.0	2.2	23.0	34.0	70.0	109	147	204	231	274
5	3.8	11.2	20.0	27.0	50.0	84	109	147	204	231
6	3.8	9.3	13.5	17.5	23.0	39	50	70	109	147
7	3.8	7.5	13.5	20.0	27.0	39	50	84	147	231
8	3.8	6.0	7.5	9.3	17.5	20	23	34	39	50
9	6.0	6.0	11.2	13.5	17.5	20	34	20	27	39
10	6.0	7.5	13.5	20.0	23.0	27	34	50	109	204
Women.										
1	3.8	6.0	9.3	11.2	11.2	13.5	17.5	23	39	109
2	2.2	3.8	6.0	7.5	9.3	17.5	34	39	50	109
3	3.8	7.5	13.5	17.5	20.0	23.0	39	50	84	198
4	2.2	6.0	7.5	9.3	11.2	23.0	39	50	84	147
5	3.8	6.0	7.5	11.2	13.5	17.5	20	27	34	50
6	3.8	11.2	17.5	34.0	50.0	70.0	84	109	147	274
7	1.7	2.2	3.8	7.5	11.2	20.0	34	70	109	198
8	3.8	7.5	11.2	13.5	20.0	17.5	27	50	109	204
9	2.2	6.0	7.5	11.2	17.5	34.0	39	50	109	198
10	1.7	3.8	6.0	7.5	9.3	11.2	13.5	17.5	23	34

Greater relative differences between the sexes are seen in case of the small weights. No woman in this group, and indeed only one in this class, selected a lead weight heavier than 3.8 grams for the equivalent of the wood disk weighing 15.5 grams. Although these results are fairly representative, it would not be safe to draw any general conclusions, regarding the relations of the factors involved, from experiments made

upon so few persons. This table is given merely as an example of individual judgments, and it depends on the contents of the following tables for its full interpretation. The average intelligent man or woman would probably judge about as did some one of these twenty persons.

Three points are made especially prominent by Table I, and all are emphasized by the tables to follow. First, the women overestimate the lead nearly twice as much as do the men. Second, the over-estimation by both sexes is greatest with medium weights, and is greater for small than for large weights. Third, the amplitude of individual judgment is so great that results from many persons are necessary to establish any relation between size and weight. In this case we have to do with the element of uncertainty on the part of each observer, which may lead him to hesitate over each of, perhaps, half a dozen lead weights for a single wood disk. Add to this immense amplitude in the judgment of every individual the enormous variation between different persons, and the difficulty in obtaining exact results becomes evident.

Consider for a moment what such a judgment means. One seeks a piece of lead which feels just as heavy as the piece of wood which he holds in his hand. If the muscle sense alone be used we are able to compare weights with considerable accuracy. From 20 grams to 1,000 grams or more even a child, with a little practice, can recognize a difference of $\frac{1}{20}$ to $\frac{1}{100}$ of the load with some certainty. With the help of all our senses it may seem as if we ought to do better. If the materials to be compared are different in specific gravity, the introduction of other senses complicates the process, and probably will always make the results less accurate. One finds himself hesitating, therefore, not merely between two weights that seem to be about equally different from the wood in hand, but one may even be in doubt as to which of half a dozen weights is nearest like the wood; and it may be that these half-dozen leads differ not by $\frac{1}{20}$ but by 2 or even 5 times the smallest. After awhile one may eliminate some of them and come to a conclusion, but seldom is he at all certain of the correctness of results. It must be said, however, that if the right weight be offered to the ob-

TABLE II.

Class of 1894.

	I.		II.		III.		IV.		V.		VI.		VII.		VIII.		IX.		X.	
WOOD.	15.5		35.		61		87.5		126.5		174		231		295		364		525	
MEN.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.
a	6.8	2.1	13.0	5.1	22.6	9.2	34.3	12.6	50.0	13.0	60.0	11.9	81.5	18.7	114.1	27.7	167.5	47.7	230.2	59.6
	5.1	1.3	13.5	7.1	21.5	7.7	29.6	10.0	40.5	11.7	54.7	18.1	77.0	31.2	104.3	47.6	153.3	63.2	205.6	60.1
	4.8	.6	9.5	1.6	17.6	1.6	25.9	3.2	35.6	3.7	48.8	5.8	63.9	3.3	98.5	29.5	152.4	43.3	229.4	62.9
b	6.6	1.6	12.6	4.3	25.0	7.7	39.9	12.3	53.4	12.1	74.8	20.6	101.4	30.8	132.6	47.4	174.0	47.4	276.0	77.1
	6.5	2.2	12.1	4.8	23.3	8.1	35.4	9.0	50.6	15.0	65.5	18.1	93.4	26.4	120.9	42.7	162.3	59.0	245.3	76.1
	5.9	2.2	10.8	4.9	18.9	7.1	30.7	9.7	42.7	12.4	53.2	13.0	73.5	25.6	113.3	41.4	166.3	68.9	229.4	69.6
c	7.4	2.8	13.3	5.0	24.6	10.8	38.6	13.7	54.7	18.8	77.4	31.9	105.5	52.1	139.5	51.0	194.2	69.9	283.2	92.4
	5.9	2.2	12.0	5.3	20.6	9.2	34.5	12.3	41.8	14.9	62.2	29.6	76.0	28.9	101.4	46.3	163.2	61.5	260.1	70.6
	7.8	3.2	15.7	5.8	27.0	10.3	40.3	12.7	54.7	17.6	75.0	29.5	103.0	39.6	137.9	40.3	186.7	47.9	266.3	75.5
WOMEN.																				
d	6.6	1.2	9.3	1.8	17.2	4.6	24.7	7.2	38.6	12.5	45.5	9.2	64.1	15.3	89.1	27.5	123.9	48.3	166.1	57.7
	5.8	1.3	8.3	1.5	16.4	4.7	22.9	5.8	31.1	6.5	42.0	7.9	52.7	11.6	64.5	7.2	82.8	17.4	122.9	29.8
	6.0	1.8	8.5	2.8	17.0	4.0	24.7	6.2	33.8	6.9	40.4	10.9	52.8	12.1	75.1	26.5	113.8	45.8	167.6	83.8
e	6.0	1.8	9.1	2.1	17.6	5.4	26.6	8.4	34.5	9.5	48.7	10.2	66.6	18.4	91.0	26.2	123.9	48.3	189.4	56.4
	4.9	1.8	10.1	3.0	18.7	4.9	23.6	3.5	33.1	5.0	50.0	14.7	54.6	11.7	89.8	34.8	106.5	46.3	164.7	61.3
	5.7	2.1	10.1	3.0	18.1	4.4	24.9	5.0	34.7	6.2	44.5	10.7	68.9	22.1	84.1	30.0	115.7	44.3	166.8	48.4
f	6.6	1.2	10.8	2.7	20.9	6.7	28.1	7.5	49.7	14.0	65.3	19.2	88.3	36.6	125.8	55.2	178.2	88.2	267.2	116.2
	5.9	1.9	11.0	4.1	19.0	7.7	28.0	10.6	39.6	11.3	51.6	21.6	75.8	37.6	115.5	44.4	158.5	66.0	210.9	66.5
	6.1	1.6	11.8	4.7	19.5	6.0	28.3	9.6	39.8	11.2	52.3	14.9	69.6	21.6	101.4	36.5	137.7	59.7	203.1	73.9

TABLE III.

Class of 1895.

	I.		II.		III.		IV.		V.		VI.		VII.		VIII.		IX.		X.	
WOOD.	15.5		35		61		87.5		126.5		174		231		295		364		525	
MEN.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.
g	4.0	1.2	8.0	2.4	13.3	5.4	19.0	8.7	30.1	13.1	41.0	18.1	63.5	26.8	99.9	41.3	134.6	56.9	204.0	73.2
	4.6	2.1	8.8	2.9	13.1	4.6	19.8	6.6	33.1	13.5	45.9	21.7	65.1	31.1	90.3	45.4	132.7	55.1	190.5	55.0
	5.0	1.9	8.3	2.6	13.6	5.1	18.1	6.7	26.7	9.7	42.3	21.0	65.1	37.7	93.1	46.9	131.5	60.5	194.6	72.8
h	4.8	1.9	10.1	5.3	19.2	10.7	27.4	15.8	39.5	25.8	58.6	36.7	83.1	49.0	113.7	57.2	170.1	73.5	239.9	96.7
	5.6	2.6	10.1	4.3	15.5	5.5	22.3	8.3	34.4	15.9	52.1	27.2	74.8	35.4	114.2	46.7	164.6	57.0	251.9	92.9
	4.6	2.2	8.9	3.2	14.4	5.5	20.6	8.6	31.8	16.1	44.2	21.9	75.0	43.0	102.4	53.3	145.5	66.3	235.8	112.0
i	3.7	1.0	7.5	1.3	11.8	3.2	18.3	6.2	26.9	10.7	36.7	13.0	58.1	23.0	79.4	32.5	121.7	47.5	199.2	82.6
	3.5	.8	7.7	1.0	10.8	1.5	15.1	3.4	20.4	4.5	31.3	7.0	49.3	16.3	75.2	28.8	123.8	49.4	184.3	54.3
	4.1	.9	6.8	1.6	11.3	2.6	15.6	4.6	22.8	5.8	36.4	9.8	58.7	19.7	88.6	24.3	140.0	36.7	214.6	32.3
j	4.5	1.2	9.1	2.3	13.5	4.5	22.5	11.1	32.8	16.4	43.7	18.8	59.3	31.8	76.3	31.4	117.5	44.7	199.5	90.0
	5.0	1.6	8.9	2.5	14.5	5.9	21.0	8.8	29.3	14.2	41.4	18.8	65.0	34.8	108.5	51.4	149.0	76.0	215.0	98.0
	6.5	2.5	12.3	6.1	16.6	7.4	24.2	10.4	34.4	15.6	47.2	22.5	68.3	38.9	110.1	52.5	167.4	57.3	268.7	119.2
WOMEN.																				
k	3.4	1.2	6.6	2.3	10.8	4.0	15.1	5.4	21.4	8.4	29.1	16.2	52.3	29.9	82.5	46.7	101.6	57.9	149.1	70.7
	5.3	2.4	9.2	3.0	15.2	4.5	24.0	7.6	32.2	11.1	45.4	20.4	64.7	27.6	94.1	34.7	122.5	44.9	171.3	55.0
	7.8	5.5	12.7	6.5	17.1	7.9	23.5	11.2	33.2	14.4	45.3	19.4	81.6	34.2	103.5	39.6	144.5	42.4	204.5	60.9
l	2.8	.8	6.2	1.4	9.7	3.5	13.4	6.0	17.1	8.3	23.0	11.8	31.8	18.2	48.0	28.2	66.6	41.6	116.4	58.8
	2.9	1.0	5.5	1.0	8.3	1.2	10.8	2.2	14.7	3.7	20.1	7.1	27.9	11.1	41.2	21.9	66.1	39.3	103.4	48.9
	3.1	1.0	6.0	1.3	8.4	2.4	11.0	3.0	14.9	5.4	21.8	9.4	32.6	15.6	49.2	26.4	74.5	43.1	126.7	67.3
m	4.2	2.2	8.5	4.1	11.7	4.8	16.1	6.8	23.2	8.6	34.5	13.5	49.1	20.7	78.8	37.3	105.8	52.0	158.2	75.8
	6.3	4.4	10.5	6.1	16.1	9.1	23.6	15.1	33.9	18.9	52.5	28.0	74.8	35.5	103.6	50.0	138.0	65.5	202.6	91.5
	4.6	2.2	8.4	3.4	14.8	9.5	19.9	12.6	26.8	16.1	36.0	21.8	56.3	35.8	80.4	46.9	109.5	55.1	157.8	78.3
n	3.2	.8	6.3	.9	8.2	1.8	11.7	2.5	16.6	5.4	24.3	5.8	42.7	12.1	58.5	15.5	85.4	21.6	163.0	45.3
	3.7	.7	6.7	1.3	9.9	1.9	14.0	1.8	19.0	2.3	29.7	8.5	39.4	10.3	60.4	22.9	103.2	37.5	185.1	69.6
	4.8	1.7	7.7	2.4	10.9	3.2	15.8	5.4	20.3	5.8	29.5	8.6	41.4	13.6	62.3	24.4	89.2	33.8	142.1	45.9

TABLE IV.

Classes of 1896 and 1897.

	I. Wood 15.5		II. 35		III. 61		IV. 87.5		V. 126.5		VI. 174		VII. 231		VIII. 295		IX. 364		X. 525	
	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.
MEN, 1896.																				
o	5.4	1.3	9.5	3.7	17.0	5.8	22.7	7.7	37.5	18.0	53.1	26.9	71.0	33.0	101.2	49.6	147.2	60.9	199.8	72.7
p	3.9	1.5	9.1	5.0	15.2	7.5	23.8	14.1	39.9	27.8	49.1	31.2	71.2	42.0	95.8	50.2	143.2	59.8	219.4	68.6
q	5.2	2.3	12.7	7.2	21.3	11.8	32.0	19.3	52.2	35.3	68.2	46.4	98.0	59.8	139.5	77.9	174.4	92.0	255.7	114.7
r	4.5	1.3	9.6	3.2	15.6	5.4	23.1	7.9	35.0	12.9	49.5	15.3	54.3	21.9	90.5	26.1	122.8	35.6	188.1	45.9
s	4.8	1.9	9.3	3.1	15.4	5.8	20.1	7.1	30.7	11.6	44.3	17.1	60.9	22.9	98.2	35.7	132.3	50.0	212.0	48.8
WOMEN, 1896.																				
t	3.0	.9	6.1	1.7	9.2	3.1	13.1	5.8	17.0	7.2	24.6	10.5	34.0	10.9	47.7	16.9	79.3	31.1	159.3	63.2
u	2.6	.7	4.7	1.5	7.9	3.4	10.9	4.8	15.0	5.9	19.3	8.1	23.0	16.5	44.2	22.2	65.4	34.3	104.1	56.1
v	2.9	1.0	6.0	2.2	9.7	3.0	13.0	5.0	19.9	9.3	29.7	15.1	44.6	21.9	66.2	34.2	108.2	58.0	160.9	81.2
MEN, 1897.																				
w	5.9	2.1	11.2	4.0	15.8	8.5	26.5	11.6	43.6	24.9	63.2	34.9	92.9	48.8	120.4	61.7	174.1	83.3	286.7	147.0
x	4.3	2.4	8.7	3.3	15.0	6.0	25.8	14.0	38.4	18.5	57.3	28.3	86.2	35.7	127.6	47.6	185.1	68.7	289.3	101.4
y	4.2	1.7	9.5	4.2	15.9	7.3	22.5	9.0	36.0	13.0	54.8	17.6	81.0	40.2	129.0	50.0	170.0	57.1	237.0	74.9
z	6.8	2.1	12.8	4.8	22.8	9.9	31.2	13.6	41.2	16.1	67.2	24.6	101.2	40.8	147.5	55.6	187.7	53.8	304.9	73.5
a′	4.3	1.4	8.9	2.2	14.2	3.6	19.6	4.0	31.6	10.2	50.3	20.2	80.7	38.6	112.0	49.7	149.1	62.1	217.0	72.9
b′	4.8	2.5	9.5	3.6	15.7	5.5	26.5	8.9	37.5	9.3	50.5	20.4	75.6	24.2	115.4	51.0	166.2	56.2	237.1	74.3
WOMEN, 1897.																				
c′	2.6	1.1	4.6	2.1	8.2	3.5	12.5	5.7	18.1	9.1	25.7	13.0	44.4	22.1	61.7	31.2	96.5	57.9	156.2	95.0
d′	2.8	1.3	4.5	1.5	7.4	1.7	10.8	3.5	17.4	8.8	23.8	12.0	31.8	18.3	45.7	32.2	62.3	48.2	94.7	75.6
e′	3.6	1.4	7.0	3.4	11.3	6.6	17.7	9.9	26.1	17.2	40.1	23.7	54.3	33.5	82.2	55.7	113.6	76.5	168.2	114.6
f′	3.3	1.6	5.8	2.3	9.6	3.3	14.6	5.8	19.2	8.6	28.3	15.0	37.7	20.8	59.6	41.9	85.9	55.3	130.1	78.5
g′	3.3	1.4	6.4	3.9	9.3	5.4	11.0	6.2	16.3	8.6	21.9	12.3	37.7	22.5	79.1	48.0	106.6	65.3	188.0	103.5

server he will reject it nine times out of ten, with every evidence of certainty in its overweight. About the only thing he *is* certain of is that the right weight is decidedly too heavy.

Table II. contains a summary of the results of these experiments with the students of the psychology class for the academic year '93–'94. Table III. contains the corresponding results for the class '94–'95. In each table the Roman numeral indicates the number of the wood disk, beginning with the smallest. The weight of the wood in grams is given just below. Then follow the results from the men in three divisions (groups *a*, *b*, *c*), and those from the women, also in three divisions (groups *d*, *e*, *f*). In Table III. both men and women are divided into four divisions each.

These two classes repeated this exercise after one week, and again after another week. The average of all first judgments for each group of students is given in the first horizontal line of numbers in that group, and the average of the judgments made a week later in the second horizontal line. The average of the last judgments is recorded in the third horizontal line of each group.

Under each Roman numeral are two vertical columns of numbers. The first column contains the average weight of the lead (in grams), which seemed to be equal to the wood disk whose weight is at the head of the column. The second vertical column contains the average variation of the individuals of each group from the average of all the members of that group.

Table IV. contains the corresponding results from the classes of '95–'96 and '96–'97. In the former year each student made only one series, and for the latter year only the first series is given. Each group is therefore represented only by one horizontal line.

It will be observed that the average of the class of '93–'94 is larger than that for any of the other classes. Especially is this true of the smaller disks. I think the difference is, perhaps, caused by the larger number of small leads provided for the later classes.

On the average the third judgment is but little greater than

the first. This exercise was given at a time when the interest of the students was held on another part of the subject, and every precaution was taken to keep them interested in other experiments. It is possible, however, that a few students may have practiced at home and thereby may have learned something of the true relations. Yet the only person whose results seem to suggest such an explanation did not do so, I am quite certain. Of course, the students did not know they were to make another series.

Table V. contains the results from the class of '96–'97 at their second trial, made a week later than the first series. The wood disks were changed a little to correspond exactly with a new series of lead weights made to correct certain observed defects in those previously employed.

The lead weights increased more regularly in weight and especially in diameter and thickness. Instead of trusting to moulds for final form and size, the mould was used only for approximate size. A file was employed to cut down the disk to the desired diameter. By this means both diameter and thickness were gradually and evenly increased, retaining approximately the relation of 2 to 1 throughout the series of leads. The weights used in this series are in grams as follows: 1, 1½, 2, 3, 4, 5, 6, 8, 10, 12, 15, 18, 21, 25, 30, 40, 50, 60, 75, 100, 125, 160, 200, 240, 300, 370, 450, 530, 625. This proportion has proved so satisfactory that it can safely be recommended. I would change it only for spheres or cubes in both light and heavy material.

Before attempting to account for the illusion here revealed, or to find a quantitative measure of the relations involved, let us notice some further details of these tables. The illusion is greater for women than for men. It is greater with small weights than with large weights. The variation of individuals is immense. The average variation in some of the groups amounts to more than half of the average for the group, though as a rule it runs from one-fourth to one-third in the first years, with a strong tendency to increase in later years. This is due, in part, to the better isolation of students while judging, and, in part, to the greater range in leads provided. The average vari-

TABLE V.

Class of 1897 (New Series).

WOOD.	I.		II.		III.		IV.		V.		VI.		VII.		VIII.		IX.		X.	
MEN.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.
A	4.2	1.8	8.0	3.0	14.1	6.1	25.9	11.5	35.5	18.7	53.1	17.1	78.5	40.9	115.5	56.6	169.0	60.6	243.5	93.9
B	4.1	1.5	8.7	3.1	13.7	5.9	24.9	10.1	35.9	15.1	49.2	17.8	74.8	33.2	110.6	24.2	161.0	51.6	238.0	68.4
C	6.1	2.3	9.9	3.3	18.5	7.1	31.0	13.4	44.1	21.7	57.0	26.0	91.0	41.0	133.0	51.0	167.5	60.5	244.0	86.0
D	5.7	2.2	9.9	2.5	17.1	7.1	29.6	14.3	41.5	22.1	63.8	33.7	98.0	39.4	133.0	51.0	179.5	76.6	264.5	116.6
E	3.9	1.7	6.8	3.0	11.2	4.4	18.1	6.7	25.8	10.2	37.2	15.4	59.1	23.1	93.5	34.8	136.0	56.0	213.0	79.0
F	3.4	1.3	5.9	1.9	10.0	4.4	17.1	5.7	23.4	8.0	39.0	19.7	66.1	35.7	104.4	50.4	147.2	78.0	241.1	121.5
WOMEN.																				
G	3.1	1.3	5.6	3.1	8.4	4.3	13.0	7.4	19.9	10.3	27.5	14.0	46.0	25.2	70.5	38.6	102.1	47.3	187.8	93.0
H	4.1	2.6	8.1	6.8	10.9	9.2	15.1	12.0	20.1	15.1	26.7	21.3	36.9	30.8	58.3	34.0	81.7	78.9	109.8	102.1
I	3.6	2.0	7.5	4.9	10.7	6.2	16.9	8.7	23.5	13.2	30.7	17.4	44.0	25.0	64.4	40.6	89.7	56.3	140.0	84.0
J	4.0	1.9	6.8	3.2	10.4	4.6	15.4	7.1	21.5	10.8	29.3	14.6	42.4	25.1	58.9	34.9	86.6	53.7	125.0	72.0
K	3.1	1.4	4.7	1.6	8.9	3.7	14.1	7.1	26.6	18.7	38.0	27.8	51.1	36.7	72.3	50.8	132.4	70.6	165.1	84.9

ation of the women is decidedly less than that of the men, though the extreme variations are, perhaps, found among the women, on account of their selection of smaller weights than any taken by the men. If it were desired to have a low variation it could easily be attained by throwing out only 10 per cent. of the persons examined. We *know* they are 'abnormal.'

There are many small inequalities in the tables, some of which could doubtless be accounted for by reference to the larger tables from which these results are taken. Such, for example, is the case with group *k*, in which for disk I. the three successive judgments show a remarkable growth. This is explained by the fact that three of the women in that group did more *thinking* between times than the course of study called for. On the whole, these tables offer enough material and good enough material to warrant some general conclusions, and to control any results of another similar investigation. Certain features have come out in the progress of the work which should be carefully considered by any one else who may wish to take up a related problem.

The series of lead weights used for most of our experiments was defective, inasmuch as the diameter did not vary regularly with the weight. Only four different diameters were found in our original series of leads. The thickness of the disks increased gradually with the weight for any given diameter, yet it happened that the heaviest weight of any diameter was *thicker* than the lightest weight of the next larger diameter. As a rule, this fact seemed unimportant, since the lead disks were plainly numbered in the order of their weight. A few students, however, were disturbed by this sudden increase in the diameter of the disks accompanied by a decrease in thickness. The change in proportion tended to produce the same illusion as that caused by difference in specific gravity. The more compact lead disks seemed heavier than the thin disk of greater surface. Our *new* series of lead weights avoids this source of error by increasing both diameter and thickness as regularly and evenly as possible.

The discussion of the primary sources of the illusion is postponed till all the evidence is before us. It may be said here

that extent of skin surface stimulated by the weights is not the principal cause of the illusion; and yet this is the explanation suggested to all who have witnessed or performed the experiments.

In a series of ten judgments such as we have in these experiments it is impossible to escape the influence of earlier judgments upon later ones. To some extent this may be observed in Table I. Such influence is very different in different individuals. For example, in numbers 7 and 8 of the men in Table I. the first judgments are exactly alike, but the last judgment of number 7 is four and one-half times as large as the last judgment of number 8. On the other hand, I have noticed a few students who assumed that their first judgments were right, and then estimated that the second wood disk bore a certain relation to the first. Their self-imposed task was to find a lead disk which bore the same relation to the first lead disk. Of course, such methods were disallowed whenever suspected. We tried to obtain an independent sense-judgment in every instance.

In many other cases there was undoubtedly a strong, unconscious influence of the earlier judgments. If a different judgment had been made at the beginning of the series, it would have caused no little divergence toward the end of the series. This factor cannot be separated from the natural tendency of the individual to make about the same proportional error on each member of the series. The contrary tendency, to make each judgment without reference to previous ones, is seen in several persons. Thus it happens that a wood disk is sometimes matched with a smaller lead than was used to match a smaller wood disk just a moment before. So marked an independence in judgment is not frequent, and it is probable that the reliance on previous judgments is less potent than the personal qualities of the student.

The further question which seemed to me to be suggested by my own experience is *the quantitative relation between size and apparent weight* when all senses are free to act as they may. For lead and wood weights and adult persons there is no need of further experiments, except in all beginning classes in psychology, where such experiments serve to impress upon the

minds of students the points studied in class and lecture room. Other materials both heavier and lighter, and other persons both younger and older, offer a field for interesting investigations and profitable results. It is proposed to carry this investigation into the lower grades of the public schools and to vary the weights from the lightest obtainable material to that equal in density to lead itself.

We shall now consider Table VI. with reference to the quantitative results of comparing the weights of lead and wood. It will be remembered that the results of the class of '93–'94 are considerably different from those of the other three years, and that the apparent reason was the small number of lead weights used in the year 1894. It is not probable that a further increase in the number of lead weights would much affect the judgments. I have therefore brought together in Table VI. the results of the *first* judgments for the years '95, '96 and '97.

The first horizontal line contains the average of all the groups of men, with the average of the average variations as given for the separate groups in the preceding tables. Both of these numbers are determined directly from Tables III., IV. and V. according to the method recommended by Fechner in case of many experiments by same observer (*Psychophysik*, 1^te^ Aufl., I., 214 f.). If these groups had each included twice as many observers the average variation would have been slightly less.

This table probably gives the true relation between lead and light wood as felt by intelligent adults. We may therefore consider its facts with reference to quantitative relations between lead and wood, and also the effect on this relation of the absolute weights involved.

With the smallest wood disk we observe that the men find 4.7 grams of lead equal to 15.5 grams of wood. The women find 3.1 grams of lead equal to 15.5 grams of wood. The men think 229.2 grams of lead feel as heavy as 525 grams of wood, while the women select lead weighing only 145.2 grams as equivalent to 525 grams of wood. The entire series may be studied in the table.

We observe also the relation of wood to lead in this table.

TABLE VI.

	I.		II.		III.		IV.		V.		VI.		VII.		VIII.		IX.		X.	
WOOD.	15.5		35		61		87.5		126.5		174		231		295		364		525	
MEN.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.	A.	V.
1895	4.3	1.3	8.7	2.8	14.5	6.0	21.8	10.5	32.3	16.5	45.0	21.7	66.0	32.7	92.3	40.6	136.0	55.7	210.7	85.6
1896	4.8	1.7	10.0	4.4	16.9	7.3	24.3	11.2	39.1	21.1	52.8	27.4	71.1	35.9	105.0	47.9	144.0	59.7	215.0	70.1
1897	5.1	2.0	10.1	3.7	16.6	6.8	25.4	10.3	38.1	15.3	57.1	24.3	86.3	38.1	125.3	52.6	172.0	63.5	262.0	90.7
AV'GE.	4.7	1.7	9.6	3.6	16.0	6.7	23.8	10.6	36.5	17.6	51.6	24.5	74.5	35.6	107.5	47.0	150.7	59.6	229.2	82.1
WOMEN.																				
1895	3.4	1.3	6.9	2.2	10.1	3.5	14.8	5.2	19.6	7.7	27.7	11.8	44.0	20.2	67.0	31.9	89.9	43.3	146.7	62.7
1896	2.8	.9	5.6	1.8	8.9	3.2	12.3	5.2	17.3	7.5	24.4	11.2	33.9	16.4	52.7	24.4	84.3	41.1	141.4	66.8
1897	3.1	1.4	5.7	2.6	9.2	4.1	13.5	6.2	19.4	10.5	28.0	15.2	41.4	23.4	65.7	41.8	93.0	60.6	147.4	93.4
AV'GE.	3.1	1.2	6.1	2.2	9.4	3.6	13.5	5.5	18.8	8.6	26.7	12.7	39.8	20.0	61.8	32.7	89.1	48.3	145.2	74-3
MEN. '95, '96, '97 WOOD. LEAD.	3.30		3.65		3.81		3.68		3.47		3.37		3.10		2.75		2.42		AV'GE. 2.25	3.18
WOMEN. '95, '96, '97 WOOD. LEAD.	5.00		5.74		6.49		6.48		6.73		6.52		5.80		4.77		4.09		3.62	5.52
MEN. WOMEN.	.66		.64		.59		.57		.52		.52		.53		.58		.60		.62	58.3

While the relation is not constant it evidently varies according to a simple law. It is not safe to say, however, that this law depends entirely upon the weight of the substances to be compared. It ought to be more generally recognized that many 'laws' of this kind have been established upon very porous foundations. It is, in fact, not improbable that a series of wood disks beginning with the size of my middle disk and extending beyond the largest of my series would show a similar law of rise and fall in the ratio of wood to lead. The constant errors arising from the order of experiments have not been eliminated in this work. The number in the series, and especially the number of lead weights at hand, probably, give rise to other constant errors.

With both men and women the ratio of wood to lead at first increases with the weight, then decreases more rapidly, till for the heaviest weight the ratio is about two-thirds as great as for the lightest weight. In case of the men the ratio increases only for the first three weights, while with the women it increases very fast for the first three weights and then more slowly for the next two, falling very little even for the sixth weight. It will be noticed that the fall in case of the men is not regularly marked until after the sixth weight is passed.

If we consider the specific gravity of the materials involved the following results are obtained. The specific gravity of the wood employed is about .45; of the lead 11.35, *i. e.*, the wood is to the lead as 1 to 25. Comparing these facts with the figures of Table VI. we find that the relations of weight and size for these materials and with these persons in the experiments already described may be formulated as follows: In equal masses the lead is twenty-five times as heavy as the wood. For equal weights the wood is twenty-five times as large as the lead. In both cases the *difference* is overestimated from the momentum of the contrasting sensations. The wood seems lighter and the lead heavier than it really is. If we ascribe one-half of the error to each substance, we find that, in comparing lead and wood weights, men estimate the lead at about twice its actual weight, and that they estimate the wood at about two-thirds its actual weight. Under the same conditions women estimate lead

at more than three times its real value and wood at less than three-fifths its real value.

Of course there is no evidence in this paper that the error is due to the two substances in equal parts. The actual relation of these factors cannot be determined from my experiments. The introduction of two or three other substances for comparison with lead would furnish necessary data for a more accurate conclusion.

Experiments with Paper Bags and Lead Weights.

It is probable that the definite relation existing between weight and bulk as estimated by intelligent persons may be easily determined. It is proposed to test this question with succeeding classes. The following experiments with extreme differences in the materials indicate the need for some determination and also the apparent simplicity of the method required for the investigation.

Two light paper bags, each with a capacity of about ten litres, were obtained from a milliner. One was filled with air so that it occupied a space of perhaps 7,000 c.cm. The other was loosely filled with cotton and appeared about the same in cubic contents. The first bag weighed 15.9 grams, the second weighed 106.5 grams. Each bag was suspended by a string from a fixed rod. A series of lead weights was placed on a table just below the bags; the leads beginning with .2 gram and increasing slowly but irregularly till they are connected with the series of leads used in the experiments already described.

The students worked on this problem singly. They were permitted to lift the weights as they pleased. As a rule, they lifted the paper bags by the compressed tops or by the strings, and the lead weights between the fingers or on the palm of the other hand, lifting both at the same time.

Here, as in other experiments on weights, an occasional student thought the result depended upon the manner of lifting the weights. While some little influence of this factor may be admitted, it was not observed to produce any effect as between different students. That is, the other factors are so much more powerful in determining the result that the method of lifting the

TABLE VII.

Paper bags compared with lead weights.

PAPER BAG = 15.875		106.5	GRAMS	15.875	106.5
MEN.	LEAD.	LEAD.	MEN.	LEAD.	LEAD.
1	3.8	11.2	26	11.2	70.0
2	0.6	50.0	27	6.0	70.0
3	6.0	34.0	28	0.6	27.0
4	1.7	17.5	29	1.0	3.8
5	8.8	70.0	30	11.2	39.0
6	13.5	50.0	31	2.2	27.0
7	1.7	11.2	32	1.0	6.0
8	1.0	11.2	33	0.6	3.8
9	11.2	70.0	34	3.8	17.5
10	11.2	147.0	35	1.0	9.3
11	3.8	7.5	36	3.8	27.0
12	6.0	27.0	37	1.7	20.0
13	2.2	9.3	38	2.2	39.0
14	3.8	20.0	39	2.2	34.0
15	2.2	39.0	40	3.8	7.5
16	3.8	50.0	41	1.7	11.2
17	3.8	27.0	42	3.8	20.0
18	1.7	11.2	43	2.2	11.2
19	2.2	13.5	44	6.0	20.0
20	3.8	50.0	45	2.2	6.0
21	3.8	34.0	46	9.3	70.0
22	1.0	11.2	47	3.8	34.0
23	3.8	27.0	48	11.2	50.0
24	2.2	34.0	49	6.0	13.5
25	0.6	27.0	50	11.2	39.0
			51	9.3	109.0
Average				4.31	32.26
WOMEN.			WOMEN.		
1	1.7	3.8	16	0.6	3.8
2	0.3	7.5	17	1.7	20.0
3	2.2	39.0	18	0.2	0.6
4	2.2	50.0	19	3.8	11.2
5	0.4	3.8	20	0.6	1.7
6	0.6	13.5	21	0.4	6.0
7	0.3	2.2	22	2.2	13.5
8	2.2	11.2	23	6.0	50.0
9	1.7	9.3	24	3.8	20.0
10	0.6	3.8	25	1.0	7.5
11	11.2	50.0	26	3.8	23.0
12	2.2	20.0	27	1.7	6.0
13	1.7	34.0	28	9.3	27.0
14	0.3	0.6	29	1.7	6.0
15	0.3	11.2	30	0.6	6.0
Average				2.18	16.08

weights exerted no appreciable influence. Furthermore, most of the students used several methods before finally deciding upon any lead. Table VII. gives the results of the individual judgments and the separate averages of men and women for the class of 1896.

The averages in this table are astounding, but some of the individual judgments are nearly incredible. The reader may acquire more faith, however, by attempting to estimate the weight of an empty paste-board box in terms of lead.

The table shows that no one selected a lead weight equal to or heavier than the paper bag. In case of the smaller bag only two women and nine men selected leads half as heavy as the paper bag, while twelve women and four men selected a lead less than one twenty-fifth as heavy as the paper bag, and five women chose a lead less than one fiftieth as heavy as the bag, the average of the five being one fifty-seventh. As equivalent to the larger bag two of the women chose a lead equal to one one-hundred-and-seventy-seventh ($\frac{1}{177}$) of the weight of the bag; while eighteen women and fifteen men selected leads less than one ninth as heavy as the bag, their average being, for the women, less than one eighteenth and for the men less than one twelfth.

The average of the men is for both paper bags almost exactly twice the average of the women, and this latter average is about one seventh of the true weight. It is evident from these results that the lighter the material the more difficult it is to make a comparison with the weight of lead.

It is proposed to attempt the formulation of the law expressing this relation from experiments planned for next year.

Table VIII. contains a condensed summary of results obtained from the classes of the past two years. The class of 1897 made two series of experiments. In the first series the leads of '96 were used. For the second series the same paper bags were employed, but the new leads of 1897 were substituted for the less regular ones of the first experiments.

For this table the students were not arranged alphabetically, as for the other tables of this paper, but they were grouped in fives, according to the weight of the lead disks selected to match

TABLE VIII.

Paper bags compared with lead weights.

	PAPER BAG = 15.9 GRAMS.			PAPER BAG = 106.5 GRAMS.		
MEN.	CLASS OF '96.	CLASS OF '97. SERIES OF '96.	CLASS OF '97. SERIES OF '97.	CLASS OF '96.	CLASS OF '97. SERIES OF '96.	CLASS OF '97. SERIES OF '97.
1st five.	.60	.78	.84	5.4	6.5	7.8
2d "	1.00	1.00	1.10	9.7	10.1	13.4
3d "	1.70	1.28	1.60	11.2	12.4	17.4
4th "	2.20	1.90	2.00	16.4	15.9	18.6
5th "	2.52	2.20	3.00	22.8	18.0	21.0
6th "	3.80	3.48	3.00	27.0	22.4	23.4
7th "	3.80	3.80	4.00	34.0	29.8	26.0
8th "	4.68	6.00	4.60	41.2	34.0	30.0
9th "	8.36	6.00	5.00	55.0	39.0	34.0
10th "	11.20	7.50	5.80	77.8	50.0	46.0
11th "		8.88	7.20		70.0	50.0
12th "		11.20	9.00		77.0	80.0
WOMEN.						
1st five.	.28	.30	.46	1.8	2.3	1.9
2d "	.52	.56	.84	4.7	3.8	4.0
3d "	1.12	.76	1.00	7.3	6.6	5.6
4th "	1.80	1.00	1.30	12.1	9.0	8.8
5th "	2.52	1.00	1.50	22.0	12.6	11.2
6th "	6.82	1.56	1.70	44.6	15.1	13.8
7th "		1.90	2.40		19.5	19.4
8th "		2.52	3.60		23.0	26.0
9th "		4.80	5.50		34.2	36.0
10th "		6.28	7.50		50.0	70.0

the paper bags. For example, the first group consists of the five men who selected the lightest lead weights as equivalent to the lightest paper bag. The average of their judgments is given in the first horizontal line. The results for the two paper bags were treated separately, hence the persons in the various groups are not the same for the two paper bags.

The only justification for such a table is lack of space to present the individual judgments. It gives a better idea of personal differences in judgments of this kind than would the aver-

ages of large groups together with the average variations of the members of the groups. There is also less danger from freaks when an average of several persons forms the basis of conclusions.

From Table VIII. it seems that about one woman in seven finds one gram of lead equal in weight to *sixty* grams of inflated paper bag, and that not one woman in seven will find a gram of inflated paper bag half as heavy as a gram of lead. Men are somewhat better than women in comparing these weights, yet one man in every ten finds a gram of lead as heavy as twenty-five grams of inflated paper bag, and not one man in ten thinks a gram of inflated paper bag is three-fourths as heavy as a gram of lead.

There is little difference in the proportionate error between the two bags, but of course two members of a series would not be sufficient to determine the relations between specific gravity and apparent weight, or the effect of absolute weight upon the judgment. It is possible that the failure of a considerable difference in specific gravity to change the constant error appreciably in case of the two bags is due to some unconscious process suggested by the similar size and appearance of the bags. That it depends upon the similarity in *method* of raising the weight I do not believe.

We need to supplement these experiments by a series upon many persons in which the size and form of the objects remain the same, while the weight varies from the lightest possible to the specific gravity of lead itself. It is already evident that the enormous error revealed by the foregoing tables is not the effect of a simple process, but that it depends complexly upon many factors. In the conclusion of this paper I shall discuss this point more at length.

Experiments with Brass Cylinders.

As already stated the problem here studied (or a problem involving part of the same factors) was attacked in a different way by Dr. Dresslar. I thought it worth while to prepare a set of cylinders somewhat like those used by Dr. Dresslar, but with the addition of three longer and heavier cylinders. My set was

one inch in diameter and from one to eight inches in length, increasing by half inches, except the last two of the series, which increased by one inch each. The first ten, beginning with the shortest, were all equal in weight (169 grams). The last three were heavier than the others, number eleven weighing 25 grams more than the first. Number twelve is 45 grams and number thirteen 75 grams heavier than the first. It will be remembered that Dr. Dresslar's cylinders were all equal in weight and in diameter, but increased regularly in length by half inches from one and one-half inches to five inches, each weighing 132 grams.

The students were requested to arrange our cylinders in the order of their weight, beginning with the heaviest. The cylinders were placed upon a table in front of which the student sat. The suggestion was given that the weights should be lifted, one at a time, between thumb and finger. Other methods of holding the weights were not prohibited. Some students in our first year's work may have lifted a weight in each hand simultaneously, though variations from the suggested method were not numerous, and in the last two years were not permitted. Each student was given all the time he desired. The average time used was between ten and fifteen minutes. The students were not asked to express a quantitative judgment, as it is believed that psychologic method offers no place for quantitative judgments of this kind. It is true, however, that we learn, after much experience, to estimate weights in terms of an absent fixed standard, but this process is always very inaccurate, and when the standard is much less or much greater than the weight in hand the results are scarcely of scientific value. Even more difficult is it to tell how many times heavier one unfamiliar object is than another equally unfamiliar and differently constructed object. A multiple of sensations does not exist as a mental product, and when we try to attain a judgment involving such a process we do so only by referring our sensations to more or less indefinite causes of the sensations. On the other hand, equality or inequality in sensation is a plain psychologic fact.

If Dr. Dresslar had compared his shortest cylinder with lead

weights and then his longest cylinder with the same weights, selecting such as seemed most nearly equal to each of the cylinders, he would probably have found quite a difference in the relation of these two cylinders from that given in his table. It is, in fact, observed in my series, in which the longest cylinders are actually heavier than the shortest, that the students apparently have made much smaller errors. That is, it does not seem probable that, if the shortest cylinder had produced two or three times as great sensation as the longest of those equal in weight, the effect of the greater actual weight of the last three would have become evident in the results. We notice in Table IX. that the first long cylinder which is heavier than the shortest produces an unmistakable effect on the arrangement, and that the other heavier cylinders show a corresponding increase in such effect. Now the first of the heavier weights (number 11) is only 25 grams heavier than the shortest. The longest is less than fifty per cent. heavier than the shortest.

Table IX. shows the results of these experiments for the three years '95, '96 and '97. The cylinders are arranged for the table in order of their length, beginning with the shortest. The numbers at the head of the table indicate order in length. Other irregular numbers were pasted on the bottoms of the cylinders for use in recording the results after the arrangement was determined. Of course, these numbers were unknown to the student until the experiment was finished.

The first thing which attracts our attention in the table is the greater compactness in the results of the women. Further examination shows that this fact is due to the greater regularity of the women in following length as index of the weight, deciding this relation, of course, in an inverse ratio. Now it may be remembered that Dr. Dresslar concluded from his experiments that the more intelligent pupils departed less from the law than those of weaker minds. We must conclude, if this be true, that the women of these classes are brighter than the men. I shall not at present offer any further evidence on this point.

In detail the table shows, for example, that 123 men and 107 women chose the shortest cylinder (1) as the heaviest; 10 men and 4 women as second heaviest; 9 men and 1 woman as third

TABLE IX.

Brass cylinders. Classes 1895, '96 and '97.

150 Men. 112 Women.

Order in Length.	1		2		3		4		5		6		7		8		9		10		11		12		13	
Sex.	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F	M	F
Order in Place.																										
1	**123**	**107**	7						1																	
2	10	4	**94**	**101**	6		4		1		1		2						1				1		17	4
3	9	1	18	6	**70**	**92**	18	4	2			1	1		2						2		7		23	7
4	1		13	3	31	13	**60**	**81**	6	7	5		1						2		4		10	4	16	4
5	1		9	2	16	3	23	15	**48**	**75**	9	3	2		1				2		10	1	11	1	10	6
6	1		2		12	2	14	8	37	13	**35**	**63**	13	9	1	1			2		7	2	12	4	20	8
7	2		3		5		13	3	26	12	28	21	**27**	**47**	3	1	1		2		6	3	13	4	14	9
8	2		1		3	1	7		8	2	25	12	20	13	**23**	**36**	2	3	1		13	2	11	7	17	19
9			1		2		1	1	9	1	15	7	30	9	21	17	**16**	**24**	12	4	13	15	25	14	9	12
10			1		2		6		3	2	11	3	29	17	29	15	10	10	13	3	17	23	20	17	5	10
11			1				1		2		11	1	15	10	34	12	24	18	**21**	**27**	23	17	12	15	3	6
12					1		2		6		5	1	6	2	21	16	33	21	21	28	**22**	**23**	14	13	4	7
13					2		1		1		5		4	5	15	14	64	36	34	19	11	10	4	14	**9**	**14**

heaviest; 1 man as fourth, 1 as fifth, 1 as sixth, 2 as seventh and 2 as eighth heaviest. The black-face figures show the number of men or women who placed any particular cylinder in the exact order of its size. That is, in column headed 8, 23 men and 36 women placed the eighth shortest weight in the eighth place from the apparently heaviest weight counting the latter as first place.

Observe the arrangement of those students who displaced shorter cylinders from the usual order. Of course, the shortest could be displaced only by making it come second or later. The other cylinders, however, might have been placed first or, at any rate, earlier in the series than the order of shortness required. This was very rarely done by the men and not half as often by the women.

Beginning with number 10 (at top of table) there is observed a change which becomes quite marked in case of the next longer cylinder and still more so in case of the last two cylinders. The last three vertical columns show one of the most interesting facts of this whole investigation. Let us remember the continuous increase in length from 1 to 13 and that the weight of the first ten is the same, while number 11 is somewhat heavier, with still further increase in 12 and again in 13. How clearly this difference has manifested itself in these results! There was no conscious reasoning in the process. The students have trusted their senses, and the factor which, I confess, had seemed to me too small to influence their judgment has very decidedly manifested itself. It shows that their discrimination is much greater than the off-hand estimates of Dr. Dresslar's school children would indicate, or perhaps it would be truer to say it shows that comparison of apparently unequal weights in terms of the lighter is exceedingly unreliable. The regularity of this curious result was entirely unsuspected until the data were tabulated in their present form at the end of the second year.

It naturally follows that the longest cylinder, which is also the heaviest, should have been most equally distributed over the entire field. This is seen to be true for both men and women. No such range of choice occurs in selecting cylinders for any particular place in the series as the horizontal lines of this table

clearly show. The only approximation is in case of the men and with horizontal lines 6, 7, 8, 9, 10. Lines 7 and 8 alone contain all cylinders, but several of the places have only one representative each. This characteristic of the table is due to the fact that the shortest cylinders, without exception, were placed near the heavier end of the series. Only one of the women places any of the shortest three cylinders below sixth place, and only twenty-seven judgments of the men on these first three cylinders fall below sixth place. The cylinders of medium length are crowded down toward the lighter end of the series.

This table contains some curious facts, and it may serve as a basis for speculation. It must not be used, however, to establish quantitative relations, because the method employed cannot yield accurate results. After the shortest cylinder, the longest but one of those equal in weight, *i. e.*, number 9, offers the most interesting results. For some reason it was generally looked upon as the lightest weight, even more so than number 10, which *a priori* consideration would have led us to expect. Number 9 was selected as the very lightest by 100 students out of 262, while number 10 was so regarded by only 53 students. It is not improbable that the influence of the longer and heavier cylinders is responsible for this interruption of the regular order. One of these two cylinders, however, is selected as the lightest in weight by 153 persons out of 262, the other 109 persons distributing their judgments among 9 other cylinders, most of them falling to the three on each side of numbers 9 and 10.

It is interesting to note that only men select a shorter cylinder than number 7 for the lightest, and that 38 women to 24 men select a longer cylinder than number 10 for the lightest. This fact is, perhaps, less indicative of female superiority than the regularity in the judgments of the women may have led us to infer.

The shortest cylinder is also the heaviest to 230 out of 262 persons. Only five women thought any other cylinder heavier. The men placed the next longer first in weight 7 times, and the longest of all the series 17 times.

The conflict between sight and the muscular sense is very unequal in the first part of this table. Sight evidently insists

upon hearing the evidence and deciding the case. In the last part of the table (numbers 11, 12, 13) the responsibility is pretty evenly divided.

Conclusion and Summary.

The facts stated in the preceding pages have become known, in a general way, to all persons from every-day experience. The explanations of the illusions are supposed to be the property of all thoughtful persons. Only the enormous size of the constant error is surprising. It is probable that all current theories of the cause contain a kernel of truth, but it is certain that the cause itself is exceedingly complex. Many students believed that the chief factor was the amount of surface on the hand which was stimulated. Others thought that the surface opposed to the air as the weights were lifted and balanced was most important. Some thought that the method of lifting the two kinds of weights was decisive. As a rule, the lead was lifted in the palm of the hand and the wood between the thumb and finger. Of course the students endeavored to avoid the influence of these disturbing factors whenever they were discovered.

For my part, no appreciable influence of any of these factors was observed. They doubtless have an influence, but the real cause is so much stronger than any of them that the result is not noticeably affected by varying these minor conditions. If both weights are held edgewise between the thumb and finger, the stimulated surfaces are approximately equal, the resistance of the air is reduced to a minimum, and yet the results are not perceptibly different from those obtained in the most extremely opposite manner.

It is not probable that the different temperatures of wood and lead affect our judgment of their relative weight to a noticeable extent. Whatever the value of this influence, it would, of course, tend to increase the size of the observed error.

If the weights be suspended by a cord held between the fingers, so as to give no intimation as to which is lead or wood, and if the eyes be closed and all other means of determining *size* be excluded, the judgment immediately improves until the error amounts to less than one-tenth of the weight;

while with a knowledge of the relative size the error may rise to three or even ten times the weight. If the weights be held in the hands, shutting the eyes has no appreciable effect. In short, if the observer *knows from any source* the size of the two weights which he is comparing, he cannot avoid the influence of this factor.

It would be interesting to determine the difference between good and poor visualizers in this respect. No experiments for this purpose have been made. In fact I have not tried to discover the relative value of the numerous factors involved in the illusion now under consideration. That the visual appearance of the substances is the chief factor I am certain, though it is at least possible that the visible appearance may in turn suggest the other sensations which contribute to the general result. It seems to be the effect of space relations, and hence would not be altogether absent in experiments on the blind, provided the subjects were allowed to handle the weights as they desired.

The experiments with paper bags and those with brass cylinders contain the same factors and show similar results. It will doubtless occur to speculative minds that the cause of these illusions is the fact that the race measured gross bulk before it counted or weighed. The continued influence of this primitive method of comparison is strikingly exposed in the results of such experiments as this paper describes. To-day children and savages avoid comparison by numbers and by weight. It is not impossible that this early predilection in favor of comparison by space relations may account, in part, for the errors in judgments of weight. Of course, such an hypothesis assumes that the conflict between the early form of exclusive space comparison and the later supplementary form of comparison by weight has not yet been satisfactorily settled, and hence the novelty is still too great for the compensating pendulum of experience properly to adjust itself. The further investigation of this question should be conducted upon a few innocent persons in as many ways as possible, and for as long a time as their innocence can be maintained.

We may fairly claim that the following points have been illuminated by the present investigation:

First. Our ordinary comparison of weights is an exceedingly complex process.

Second. Our experience with substances differing in specific gravity has not enabled us accurately to estimate their weight.

Third. Discrimination of weight decreases from $\frac{1}{20}$ of the load in similar substances to 7 times the load in substances considerably different in specific gravity (or, if we consider extreme cases, it may vary from $\frac{1}{100}$ to considerably over 100 times the standard).

Fourth. It is probable that the spatial senses of touch and sight have largely usurped the functions of the sense of weight, or else the latter is relatively a new sense and has not yet come to its full inheritance.

Fifth. The illusion of weight increases with the difference in specific gravity of the substances compared.

Sixth. Men are less prone than women to illusions of weight.

Seventh. The judgments of different persons may vary enormously, but as a rule the average of ten persons selected at random is fairly constant.

Eighth. The size of the error in case of any individual seems to be a function of his personality. It may often be foretold approximately, and is not much diminished by practice.

Ninth. Even knowledge of the true relations does not remove the error of the sense-judgment, though a person may learn from appropriate experience to make suitable allowance for the error of the senses.

STUDIES FROM THE HARVARD PSYCHOLOGICAL LABORATORY, X.

COMMUNICATED BY PROFESSOR E. B. DELABARRE.

Effect of Study for Examinations on the Nervous and Mental Condition of Female Students.

By Frances M. Drury and Clara F. Folsom.

In order to ascertain whether abnormal nervous conditions prevailed among the students of Radcliffe College during the mid-year examinations, we undertook a series of experiments on 25 subjects. The experiments were made first under normal conditions, and then again during the mid-year period. They consisted in a test for steadiness of muscular contraction, a memory test, a fatigue test and a test for discriminative ability. These tests were usually given in the following order: (1) the test for steadiness, (2) fatigue test, (3) test for steadiness again (to notice any difference caused by mental fatigue), (4) memory and (5) discriminative ability.

The apparatus used throughout was simple. For the steadiness test the instrument consisted of a brass plate pierced by five holes with graded diameters, which could be adjusted on a standard to the height of the subject. This plate was connected by a battery with a steel pin in such a manner that the circuit was completed when the pin touched the edges of the holes. Every time the circuit was completed in this way a sounder indicated it by a click. The subject held the pin in each hole for 30 seconds, using the right hand first, and beginning with the smallest hole. Great care was taken that the pin should not be allowed to rest against the side of the hole, or get its point stuck in the wood behind the plate. By counting the number of clicks it was ascertained how many times the pin touched the edge of the hole. Giving this test before and after the fatigue test, it showed us very plainly whether mental fatigue affected steadiness of hand. The general result was that the hands of the subjects were steadier after the mental strain.

The fatigue test consisted of 20 minutes of mental arithmetic. A continued series of numbers was dictated, to be added and subtracted until the subject lost count. The number of figures added and the correctness of the result were then noted. Some subjects were able to do long series, in some cases going as high as 200; but in many cases the reckoning would be lost after 6 or 7 numbers. A new series was then given, followed by others, until the 20 minutes were completed. In all but two or three cases the girls disliked this test very much. They complained that it made them tired, made their heads ache, and particularly that it made them nervous. For one subject we were obliged to stop the test, as she was too nervous and the irritation was too severe.

After the test for steadiness was again given, a memory test was tried. A set of 12 cards was employed, each card containing 5 'nonsense syllables.' The cards were presented one by one to the subject, who read the syllables through once and wrote down all she could remember, marking the ones she felt sure were right. This experiment was quite a favorite, because many of the girls like nonsense-jingles and rhythms. They were also always curious to see how many they got right.

For discriminative ability we used 25 lines of different lengths. These lines were on pasteboard cards, and the subject saw them separately. After drawing one, the next was shown, so that she had no way of comparing any two lines on the cards. Artistic ability or practice did not seem to have any effect on the subject's judgment in drawing the lines. Quite frequently they were conscious of over-estimating or under-estimating.

In making the experiments the subjects were encouraged to talk a moderate amount, and any remarks apropos of the test showing impressions or introspections were recorded. Many things, which did not particularly affect this experiment, appeared in the course of the tests, that made the experiment always interesting. It took about an hour to do the tests thoroughly, and the interest of the young ladies which they manifested by giving their time was heartily appreciated. The tests were given 4 or 5 times each during the early autumn, and once during the mid-year examination period, January 26 to February 15.

Specimen Records.

(1) *Memory Test.*

x—The syllables the subject was sure of.
c—The syllables that were really correct.

Miss E. C., November 2, normal condition.

cx	c	c	c	c
zod	zid	med	zal	peg
cx	c			cx
heg	nim	pod	wol	zed
c	cx	c		
lom	don	nop	pal	wid
cx	c			
lan	zek	caz	—	—
cx	xc	xc	c	c
dup	fex	paz	rud	vud
xc	xc			c
cug	maz	loz	cad	yul
xc	c	c	c	c
beb	nuz	teg	wup	zuf
xc	c			
lug	buz	rem	vav	zeb
xc	c	c	c	c
sof	hib	laz	naz	gaz
xc	c	c		c
cuk	diz	wex	yod	zab
xc		c		
nis	rud	zem	vis	zib
xc				c
nem	wog	zim	nul	wap

(2) *Memory Test.*

Miss F. K., February 3, mid-year period.

cx				
cug	ma		g	yal
cx				
dub	fe			vud
cx				c
cuk				zab
cx				c
beb		c		zuf
cx				cx
hig				zeu
cx				xc
nis				yim
				cx
l				lus
cx		c		cx
lom		nop		wim
		c		cx
		zul		wap
cx		c		c
sof		naz		laz
cx		c		
zod		pez		
c		c		
heg	kig	pol	zim	dib

Note.—"She always tried to remember all the letters. Never thought of considering them as syllables. Never thought of the sound. Says this is typical of herself. Always goes the longest way round."

This is interesting as an exception. Most subjects remembered the syllables by their sound and jingle and kept repeating them over to themselves.

(3) *Fatigue Test.*

Miss G. L., February 5, mid-year period. First column shows the number of figures added; second column shows whether result was correct or not.

20	x
22	x
6	c
31	c
32	x
17	x
8	c
32	x
17	c
39	c
21	c
7	x
8	c
5	c
31	x
10	c

NOTE.—"Shut her eyes tightly and covered them with her hands. Rocked back and forth. Buried her face on desk, scowled, squeezed her hands, put her hands behind her neck."

(4) *Fatigue Test.*

Miss F. S., February 3, mid-year period.

9	c
80	x
63	x
62	c
65	x
20	x
56	x
20	c
9	c
10	c
14	x

NOTE.—"In a happy mood, and finds it hard to keep her mind on the addition. Thinks of nice sunny days, examinations over, going home, etc. Keeps her eyes closed, and her eyeballs travel up or down, as the experimenter says plus or minus."

EXPLANATION OF THE TABULATED RESULTS.

The results of these tests are shown in detail in the accompanying table. At its extreme left is the subject's name, with

her year in college. The two rows of figures opposite her name, with *av.* and *mid.* before them, designate that the figures of the first are the average of the experiments taken in the fall, and 'Mid' are the results obtained during mid-year examinations.

In steadiness the first two columns are for the right hand, the second two for the left. The first column shows the results for the experiment before the fatigue test and the second column shows it for afterward. The third and fourth show the same thing for the left hand. H. 1, H. 2, H. 3, above the line of figures, designate the three smallest holes of the brass plate, which alone are tabulated; and the numbers given under them show the number of contacts in each hole during thirty seconds. The fifth column shows the difference between the numbers resulting before fatigue and those resulting after, for the right hand. Column six shows the same for the left hand.

In the seventh column are the records from the fatigue test. The first line of figures gives the number of series that the subject added; the second gives the average length of the series added; the third shows the number of correct series; the fourth shows the average number of figures contained in the correct series; and the fifth shows the proportion of correct to incorrect series.

The eight column is for the memory test. The first line shows how large a percentage of the syllables shown the subject attempted to write down from memory; the second shows the percentage written correctly; the third shows the actual number correct; the fourth the number of those written that the subject felt confident were correct; and the fifth the number of those estimated correct that were correct.

The ninth column is for the discriminative ability test. The lines are tabulated as either long, medium or short. The record u. e. stands for under-estimation, and o. e. for over-estimation.

From examination of this table it will be seen that during mid-years:

(1) *In Steadiness.*

9 showed greater steadiness.

Table I.

		Steadiness.																Fatigue.					Nonsense Syllables.					Lines.		
		Right. before.			Right. after.			Left. before.			Left. after.			Right. difference.			Left difference.			No. of Ser.	Av. Len. of Ser.	No. of Cor. Ser.	Av. Len. Cor. Ser.	Per Cent. Cor. Ser.	Per Cent. Writ.	Per Cent. writ. c	No. c	No. Esti. c	No. est. c were c	Long.	Med.	Short.
		H.1	H.2	H.3	H.1	H.2	H.3	H.1	H.2	H.3	H.1	H.2	H.3	H. 1	H. 2	H. 3	H. 1	H. 2	H. 3												
E. F. sp.	Av.	59	30	9	69	28	5	57	37	21	60	30	13	—10	2	4	— 3	7	8	14	38	7	26	.44	.65	.48	19	28	16	u.e.	u.e.	u.e.
	Mid.	49	27	7	36	25	4	74	27	6	54	25	13	—13	2	3	0	2—	7	9	50	7	55	.78	.58	.60	21	14	20	o.e.	o.e.	o.e.
B.W. sp.	Av.	48	23	6	33	20	4	52	28	6	41	23	2	15	3	2	11	5	4	22	9	14	7	.63	.70	.52	22	—	—	u.e.	u.e.	u.e.
	Mid.	50	28	7	53	16	8	47	19	5	39	10	0	— 3	12	—1	6	9	5	29	9	16	7	.55	.74	.63	28	—	—	u.e.	u.e.	u.e.
R. D.	Av.	44	19	3	39	16	1	48	23	7	48	17	7	5	3	2	0	6	0	27	8	23	8	.85	.84	.80	43	28	26	u.e.	u.e.	o.e.
	Mid.	49	16	2	44	13	0	40	26	8	34	15	5	5	3	2	6	11	3	29	6	24	5	.82	.86	.92	48	34	33	u.e.	u.e.	u.e.
J.W. '99.	Av.	68	33	10	57	19	7	55	33	10	54	20	14	11	4	3	1	13—	4	22	14	12	11	.54	.71	.41	18	22	16	u.e.	u.e.	o.e.
	Mid.	48	14	5	47	7	1	54	19	14	41	22	10	1	7	4	13—	4	4	15	21	8	16	.53	.75	.64	27	—	—	u.e.	u.e.	o.e.
C.M.'00.	Av.	51	21	7	58	14	6	45	33	10	49	29	12	— 7	7	1	— 4	4—	2	15	35	10	27	.66	.74	.74	33	25	24	u.e.	u.e.	o.e.
	Mid.	49	19	14	64	25	4	34	30	5	53	20	8	—15	—6	10	—19	—10—	3	7	57	4	72	.57	.70	.78	33	25	24	o.e.	o.e.	o.e.
B.H.'00.	Av.	41	20	4	45	20	9	39	30	4	33	12	9	— 4	0	—5	6	18—	5	14	34	9	30	.64	.68	.80	33	36	30	o.e.	o.e.	o.e.
	Mid.	44	25	12	54	12	13	37	13	8	46	26	17	—10	13	—1	— 9	—13—	9	11	45	5	26	.45	.80	.77	37	34	36	o.e.	o.e.	o.e.
F.L. '00.	Av.	38	16	2	31	8	2	51	21	7	44	19	6	7	8	0	7	2	1	16	26	9	23	.56	.67	.54	66	92	63	o.e.	o.e.	o.e.
	Mid.	41	9	4	24	9	2	34	15	2	23	9	1	7	0	2	11	6	1	15	27	10	23	.66	.71	.67	29	37	28	u.e.	o.e.	o.e.
G.L.'98.	Av.	43	19	7	47	12	4	51	33	22	55	23	28	— 4	7	3	— 4	10—	6	19	13	12	11	.63	.86	.55	31	46	29	u.e.	u.e.	o.e.
	Mid.	38	16	5	48	19	9	46	14	7	39	21	7	—10	—3	—4	5—	7	0	16	19	9	16	.56	.86	.55	29	37	28	u.e.	o.e.	o.e.
A. J. '00.	Av.	25	6	2	13	1	½	21	9	2	13	3	1	12	5	1⅔	8	6	1	10	76	4	61	.40	.78	.86	41	33	32	u	e.	u.e.	u.e.
	Mid.	10	1	4	2	1	1	8	4	3	10	5	0	8	0	3	— 2—	1	3	2	232	1	155	.50	.85	.84	43	41	38	u.e.	u.e.	u.e.
A. S. sp.	Av.	26	25	9	47	15	12	36	13	13	36	26	11	—11	10	—3	0—	13—	2	20	18	8	8	.40	.72	.52	25	18	12	u.e.	u.e.	o.e.
	Mid.	56	20	7	60	23	16	27	6	5	54	39	21	— 4	—3	—9	—27	—33—	16	23	19	12	19	.52	.48	.73	21	—	—	u.e.	u.e.	o.e.
B.D.'97.	Av.	25	2	⅔	12	1	0	27	5	1	18	3	1	13	1	⅔	9	2	0	8	92	6	87	.75	.90	.77	45	59	53	u.e.	u.e.	u.e.
	Mid.	43	2	1	17	0	1	24	0	0	12	0	0	26	2	0	12	0	0	7	78	3	114	.42	.91	.58	32	39	24	u	e.	o.e.	u.e.
H.T. sp.	Av.	51	9	2	43	8	2	42	10	3	39	10	2	8	1	0	3	0	1	17	40	9	34	.49	.81	.49	24	19	18	u.e.	u.e.	o.e.
	Mid.	62	17	3	78	14	1	48	21	3	49	19	3	16	3	2	— 1	2	0	16	28	7	30	.44	.88	.60	32	27	22	u.e.	u.e.	u.e.

TABLE I.—*Continued.*

		STEADINESS.																FATIGUE.					NONSENSE SYLLABLES.					LINES.		
		Right. before.			Right. after.			Left. before.			Left. after.			Right. difference.			Left. difference.			No. of Ser.	Av. Len. of Ser.	No. of Cor. Ser.	Av. Len. Cor. Ser.	Per Cent. Cor. Ser.	Per Cent. writ.	Per Cent. Writ. c	No. c	No. Esti. c	No. est c were c	Long.	Med.	Short.
		H.1	H.2	H.3	H.1	H.2	H.3	H.1	H.2	H.3	H.1	H.2	H.3	H. 1	H. 2	H. 3	H. 1	H. 2	H. 3												
E. C. sp.	Av.	59	9	2	58	8	1	45	18	3	45	9	6	1	1	1	0	9—	3	22	17	11	16	.51	.90	.70	57	19	19	u.e.	u.e.	o.e.
	Mid.	39	7	11	36	10	4	50	16	13	36	3	0	3—	3	7	14	13	13	18	18	11	20	.61	.90	.83	45	37	33	o.e.	u.e.	o.e.
E. C.'97.	Av.	40	26	7	52	24	4	49	24	7	39	23	9	—12	2	3	10	1—	2	5	134	3	95	.53	.73	.59	26	32	20	u.e.	u.e.	u.e.
	Mid.	44	23	9	47	14	0	46	36	4	44	9	2	— 3—	13	5	3	5—	2	4	172	3	174	.75	.82	.67	33	43	32	u.e.	u.e.	u.e.
M.C.'97.	Av.	82	46	13	70	31	13	65	29	10	60	38	10	12	15	0	5	9	0	9	67	5	68	.70	.75	.77	35	30	28	u.e.	u.e.	u.e.
	Mid.	68	27	5	45	12	4	43	9	0	46	16	1	23	15	1	— 3—	5—	1	8	64	6	65	.75	.82	.82	40	34	31	u.e.	u.e.	o.e.
F. F. sp.	Av.	59	19	4	50	16	3	43	12	6	61	13	4	9	3	1	—18—	1	2	17	21	11	21	.66	.55	.78	26	24	20	u.e.	o.e.	u.e.
	Mid.	38	14	3	41	8	3	51	23	3	52	14	11	—13	6	0	— 1	9—	8	19	20	10	24	.53	.65	.77	30	36	28	u.e.	o.e.	o.e.
B.H.'97.	Av.	52	13	4	49	19	9	63	23	9	53	23	11	3—	6—	5	10	0—	2	14	50	7	24	.48	.88	.66	35	46	32	u.e.	o.e.	u.e.
	Mid.	54	31	7	41	12	5	59	30	11	44	30	11	3	19	2	15	0	0	8	72	6	74	.75	.82	.77	38	—	—	u.e.	o.e.	o.e.
M.K.'97.	Av.	44	14	5	23	6	3	32	11	5	25	11	3	21	8	2	7	0	2	19	16	13	14	.70	.66	.49	23	17	13	u.e.	u.e.	o.e.
	Mid.	43	19	6	45	5	1	47	13	6	45	12	2	— 2	14	5	2	1	4	17	19	13	20	.76	.60	.61	22	14	12	u.e.	u.e.	o.e.
A.M. sp.	Av.	51	11	7	41	15	5	44	16	8	46	23	9	10—	4	2	— 2—	7—	1	36	9	10	10	.28	.48	.56	14	19	12	u.e.	u.e.	o.e.
	Mid.	46	20	9	33	18	14	48	23	15	46	23	16	13	2	—5	2	0—	1	36	7	18	6	.50	.38	.87	20	23	20	u.e.	u.e.	o.e.
S.O. '97.	Av.	35	14	2	34	11	2	34	14	3	26	10	3	1	3	0	8	4	0	18	18	15	18	.84	.67	.53	21	—	—	u.e.	u.e.	o.e.
	Mid.	38	14	1	24	8	3	49	21	1	34	1	1	14	6	—2	15	20	0	25	19	21	17	.84	.70	.52	22	—	—	u.e.	o.e.	o.e.
C. P. sp.	Av.	40	14	8	37	10	5	38	28	13	33	18	7	3	4	3	5	10	6	20	16	14	15	.69	.53	.79	25	18	17	u.e.	u.e.	o.e.
	Mid.	44	16	11	49	20	4	47	16	16	53	27	10	— 5—	4	7	— 6—	11	6	17	20	13	20	.76	.67	.60	24	11	10	u.e.	o.e.	u.e.
K. S. sp.	Av.	68	30	16	51	17	10	67	39	11	62	32	11	17	13	6	5	7	0	16	32	7	13	.43	.69	.75	31	26	23	o.e.	o.e.	o.e.
	Mid.	47	30	1	41	15	2	64	14	1	51	24	6	6	15—	1	13—	10—	5	22	15	14	18	.64	.65	.72	28	34	25	o.e.	o.e.	u.e.
M.V.'97.	Av.	59	14	3	25	9	1	43	15	4	36	3	3	34	5	2	7	12	1	20	19	12	20	.58	.62	.61	23	26	17	u.e.	u.e.	u.e.
	Mid.	33	20	10	49	14	13	38	23	18	38	15	11	—16	6—	3	0	8	7	21	16	13	18	.62	.63	.68	26	27	23	o.e.	o.e.	u.e.
D.D.'99.	Av.	66	36	20	63	35	13	36	31	14	30	29	11	3	1	7	6	2	3	24	18	12	21	.73	.67	.75	30	36	29	o.e.	o.e.	o.e.
	Mid.	40	17	2	47	14	6	55	35	18	47	22	10	— 7	3—	4	8	13	8	20	17	17	17	.85	.73	.77	34	37	29	o.e.	o.e.	o.e.
F.K. '98.	Av.	67	27	3	37	21	3	51	32	13	60	27	11	30	6	0	— 9	5	2	13	33	8	27	.58	.45	.56	15	15	12	u.e.	u.e.	o.e.
	Mid.	52	12	2	35	7	1	47	7	2	38	14	1	17	5	1	9—	7	1	11	37	5	22	.46	.47	.86	24	13	13	u.e.	u.e.	o.e.

5 " no especial change.
11 " less steadiness.

(2) *In Fatigue.*

12 showed improvement,—*i. e.*, greater capacity for mental arithmetic.
5 " no especial change.
8 " less capacity for mental arithmetic.

(3) *In Memory.*

18 showed improved memories.
7 " worse "

(4) *In Discriminative Ability.*

10 over-estimated lines more than normally.
11 kept their normal tendencies.
4 under-estimated lines more than normally.

From these results we decided that the nervous condition of the subjects was, to a slight degree, less steady during mid-year examinations than it was normally. This is undoubtedly the effect of constant and fatiguing over-study just before examinations.

The mental condition, however, was much improved, and it would seem natural that the subjects could think more quickly and more surely at such a time. Moreover, if a happy mood conduces to general expansion and over-estimation, our test for discriminative ability shows plainly that the Radcliffe students are not very much depressed by their examinations.

STUDIES FROM THE PRINCETON PSYCHOLOGICAL LABORATORY.

VIII. A Study of the Temperature Sense.

PRELIMINARY REPORT.[1]

BY J. F. CRAWFORD.

This study was carried on in the Princeton Laboratory during the year 1896–97. To call out temperature sensations brass cylinders were used 9 cm. long and 1¼ cm. wide, tapering at the last 12 mm. on each end. Two pairs of cylinders were used; in one pair the ends are ½ mm. across, in the other considerably less. More minute work can be done with the latter pair; but the pressure must be very light to prevent pricking, and the stimulus becomes correspondingly weaker. A still sharper point will not work. A needle point, for example, even at extreme temperatures, calls out no sensation but pricking (or burning). Any finer exploration than can be made with these cylinders therefore seems impracticable. A pair of cylinders is used, the one not in use being placed in water of the desired temperature, and the two being frequently exchanged. The temperature is thus kept nearly constant. Around each cylinder are two belts of cork, which serve conveniently as handles, and prevent the cylinder from heating or cooling rapidly

[1] As Mr. Crawford's work is necessarily suspended for the time this brief account of his method and results is now published. The results are so opposed to those of earlier investigators, while the method has such evident advantage over theirs (the exact reexploration of each skin-field, made possible by the device of transparent transfer-frames) that their importance, if confirmed, is at once evident. Mr. Crawford has abundant records, to my mind, to show that sensitiveness to temperature goes by areas and not by spots; for when the transfer-frames of the same area are superposed (this cannot be done by any other method) the results are: (1) clear agreement in the successive experiments as to the separate grouping of cold and hot regions respectively, and (2) a filling in of the area for either cold or hot over the whole surface. This latter would seem to be impossible were the 'spot' theory true.

J. Mark Baldwin.

through contact with the hand. Slight changes of temperature are insignificant, especially with cold, because a difference of even 5° C. makes no practical difference in the reactions. From 0° up to 23° or 24° there are cold reactions, intense below 6° or 8°, and growing less marked above 15°. At 22° hot reactions begin to come in, but are not marked till 40° or 42°. At between 49° and 54° the heat passes over into pain, which arises from all points of the skin alike. The range of cold reactions it is seen is much greater than that of hot. No tests were made below 0°.

When a response to temperature stimulation is given, the place is marked with ink made of diamond dyes, and applied with a fine hard-wood toothpick fixed in a penholder. With this arrangement it is possible to make a dot as small as necessary and to locate it very accurately. In order to get a *permanent* record of the results after a region has been explored I use what I call a 'transfer-frame.' This is like a window of architect's paper on a frame of cardboard of convenient size (1 by 1½ inches inside and ½ inch wide). The results of each sitting are exactly traced on one of these transfer-frames, and this permanent record can be compared precisely with any or all other records. With the use of these transfer-frames the skin can, after each sitting, be washed clean, and a new exploration can be made without reference to old markings. The importance of this will be noticed below. In order to make results exactly comparable small identification marks are tattooed on the skin and copied with each record on a transfer-frame. With every such record of course there is kept a written record of all the conditions of experimentation, points of interest, etc.

The chief object of the work so far has been to determine whether the sensitiveness to temperature is found only in minute spots or in continuous regions, and in either case whether any topographical law of distribution could be discovered. I have had six subjects, three of whom gave me about thirty sittings each, and three about eight. Among the first three I include myself. The result of nearly 120 sittings has been less definite than was hoped; but enough has been done to establish a method of procedure, to indicate some conclusions, and at least to show the inadequacy of some of the generally accepted work.

There is much room for skill in the manipulation of the experiments, and therefore a large personal factor in their results; a factor which can be ruled out only by repeated and painstaking experimentation. Steady hand and eye are needed. The force of suggestion can be eliminated for the subject by keeping one set of cylinders in hot water and another in cold, without letting the subject know in any given case which is being used; and for the operator by repeating the experiments *de novo* and comparing records afterwards. Results do not emerge mechanically, but at best reliance must be placed on the skill and care of the operator.

To draw the cylinder along the surface of the skin, even slowly, gives unsatisfactory results, because there are misleading effects both of inertia and of after-images, because the subject's attention is distracted by the motion, and because a comparison of temperatures is rendered difficult. It is better to set the cylinder down on successive points, advancing it in parallel lines. The points of contact can be made as close together as the cylinder end allows. The result, of course, must appear in the form of dots, and may seem to beg the question at issue. But this can be determined by the degree of correspondence of different records made on the same territory discovered by the superposition of the transparent transfer-frames of the same region upon one another against a light background.

The results of these experiments indicate clearly that the sensitiveness to temperature appears not in groups of minute spots, separated by a non-sensitive background, but in *continuous regions* of varying size and indefinite limits. Within such sensitive regions are often smaller regions of greater intensity. But these never appear in the form of a group of spots. They are continuous so far as they extend, and variations of intensity are more or less gradual.

The evidence for this conclusion rises chiefly out of a comparison of records made from the same region. From the nature of the method the single records appear in the form of groups of dots; but, while in the different records the groups of dots correspond very closely, the individual dots do not correspond at all. Within a fairly sensitive region it is impossible to find any spots that are non-sensitive.

Since these conclusions are directly opposed to those of Goldscheider,[1] who, nevertheless, cannot be suspected of careless work, some explanation is demanded of how he could arrive at his conclusions. Any one following out with care the method sketched above will, I believe, soon make the matter clear to himself, though it is not easy to explain it without actual manipulation. Since any method of recording gives the result in the form of dots, and since slight errors on the part of both operator and subject are constantly involved, the result of a *single sitting* will be a definite arrangement of dots in lines and groups. If now the experiment is repeated *with the old markings on the skin*, which, if I understand him, was Goldscheider's method, one is irresistibly led to follow out the old tracks, and to find approximately the same dots as before, when one ought really to find different dots in the same group. But a method by which one begins the second series *de novo* frees one from the thralldom of the old marks, and a comparison of the results shows that Goldscheider's assumption of minute persistent points in a non-sensitive field must be replaced by that of continuously sensitive regions. The same objection may be made to the results of Kiesow,[2] and a different objection to those of Donaldson[3] (on the ground of continuous motion and variations of pressure). My results are much closer to the original results of Blix. Dessoir[4] discovered the incorrectness of Goldscheider's topography, but his sweeping denial of all variations of topography is absurd and has been fully met by Goldscheider himself.

I do not yet feel justified in stating any other conclusions as proved. In the size, arrangement and relative intensity of regions I have found no law. The relation between 'hot' and 'cold' seems to be one of mutual independence. They are neither coincident nor complementary, but seem to overlap without law. But these and other questions need to be much more fully pursued. Especially difficult and important is it to determine how long the effects of inertia and after-images last, and

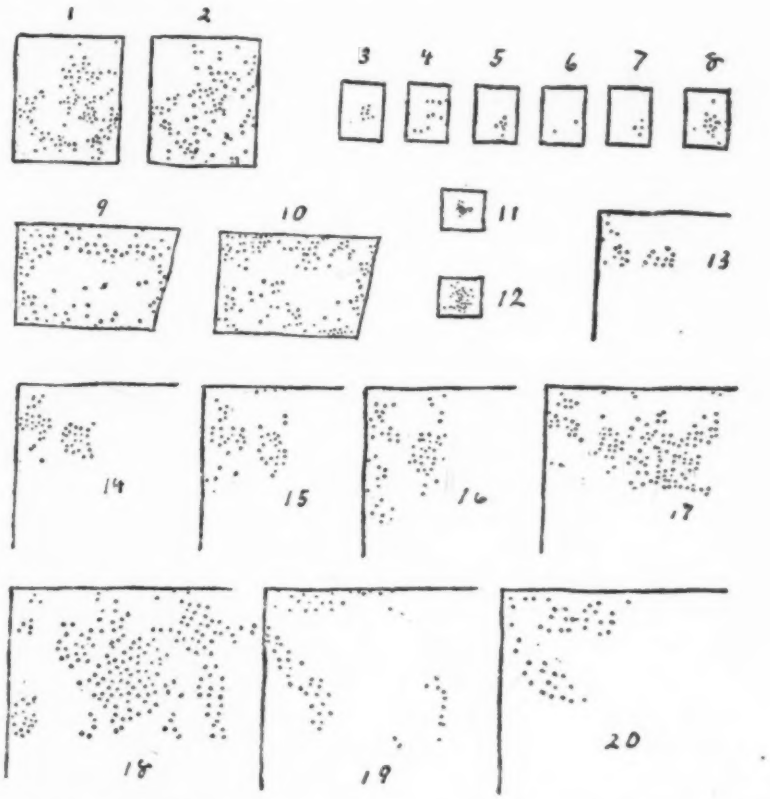
[1] Du Bois-Reymond's *Archiv*, 1885. *Suppl. Bd.* 1–110., *et al.*

[2] *Phil. Studien.*, XI.

[3] *Mind*, X., 399–416.

[4] Du Bois-Reymond's *Archiv*, 1892, 175–339. See Goldscheider's *Criticism*, *Zeitschr. f. Psych. u. Phys. d. Sinnesorgane*, 1893, 117–122.

whether they extend beyond the point of actual stimulation. Till this is determined records cannot be very sure. Later I hope to give a report of more extended work, accompanied by plates containing detailed evidence taken from the record-frames, and by a full discussion of literature. The plate accompanying this paper is merely illustrative.



Explanation of plate. (1)-(2). Two explorations for cold, same region, on *K*. (3)-(8). The same 'hot' region at six sittings, on *K*. (9)-(10). Cold, repetition on same region, on *M*. (11). Attempt at minute exploration. It is not possible to find non-sensitive regions *between* these dots. (12). Similar. Both on *M*. (13)-(18). Same region on *C*, cold, but exploration becoming more extensive. (19)-(20). Same region, but with *insensibility* to cold marked. These should be nearly complementary to the others. Note that by this method groups of dots represent continuous regions, not isolated points.

DISCUSSION AND REPORTS.

SOCIAL CONSCIOUSNESS IN CHILDREN.

The syllabus of child study prepared by Professor Baldwin for the Illinois Society for Child Study suggested to me a study of one phase of the social consciousness of children—namely, the particular traits and habits emphasized by children in the selection of a chum. This question was given in the form of a language lesson to 2,336 school children in western Massachusetts: What kind of a chum do you like the best? Papers were collated from 1,068 boys and 1,268 girls, and the ages ranged from 7 to 16 years inclusive.

The characteristics enumerated were varied and numerous, and rather forcibly suggest the indices of character mediating the child's likes and dislikes. These were collated under six general headings—age, sex, size, physical traits, habits, mental and moral traits.

Age is mentioned by 134 of the children—oftenest by the older children and more than twice as often by the girls as by the boys. An older chum is wanted by 27 of the children and a chum of the same age as themselves by 127. None of them desire a younger chum.

Sex is mentioned much oftener, being given by 611 of the children, most of them wanting a chum of their own sex; among the girls, 28 want boy chums and 328 want girl chums; with the boys, 20 prefer girl chums and 235 boy chums. The girls who want boy chums are over 9 and under 13 years and the boys who want girl chums are over 10 and under 14 years.

Size is less important than age; 58 of the children want a chum of their own size and 31 desire a chum who is larger; none of them want a smaller chum. Size is an essentially masculine characteristic. Of the 31 who desire a larger chum, 24 are boys. This particular characteristic appears oftenest from 10 to 14 years.

Seventy of the children—55 girls and 15 boys—want a chum that is quick and sprightly; thirty-eight of the children have color preferences in the matter of hair and eyes, of whom 32 are girls and 6 are boys. Most of the preferences are for blue or hazel eyes and fair or flaxen hair; but one lad of 12 years specifies that he does not want a

chum with red hair. Fourteen girls and five boys want chums that are pretty or handsome.

Peace, tranquillity, harmony and good will are qualities highly esteemed by these western Massachusetts boys and girls if one is to judge by the large number (418) who want a chum that is not quarrelsome; and this quality of temperament is more highly esteemed by the girls than by the boys. It is oftenest mentioned by children from 9 to 13 years of age.

One is not surprised to find the play instinct strong and that it should be more highly regarded by the boys than by the girls. Two hundred and seventy-nine of the children—161 boys and 118 girls—want a chum that is fond of play, and a few of the children qualify their requirement by the selfish statement that they want chums fond of playing the things they like best to play.

In the matter of good manners the girls altogether outdistance the boys. Politeness is exacted by 128 girls and by but 47 boys, although the latter are inclined to be more concrete and definite in their exactions. One boy, for example, wants a chum that will be, 'too polite to step on anybody's toes,' and another requires that he shall 'take off his hat when he comes in the house.'

One hundred of the children—64 girls and 36 boys—for the most part from 11 to 13 years of age, want chums who can 'keep secrets.' Those addicted to the use of tobacco and alcoholic drinks are contrabanded by 91 boys and 8 girls; and 73 boys and 17 girls want chums who do not swear. Clean, neat, well-dressed chums are desired by 65 girls and 22 boys; while 36 girls and 23 boys want chums who study hard, get their lessons and stand well in their classes. If the influence of teachers in forming the child's social consciousness is to be measured by this last characteristic it would not seem to be very large.

The moralities are expectedly large in the catalogue of desirable traits of character. Kindness heads the list, but this includes kindness to beasts as well as to men and children. This virtue is named by 290 girls and 188 boys, and is the one trait of character oftenest mentioned by the younger children. Three hundred and sixty-nine of the children want a chum that is cheerful and good-natured—'a jolly chum,' as some of the younger children state it. A chum that is honest and fair—one that 'don't cheat at marbles' or 'say you are two strikes when you are only one'—is described by 242 girls and 213 boys. Truthfulness is mentioned by 195 girls and 123 boys; and, like kindness, it appears frequently among the answers of the younger children.

Constancy, it should be noted, is not alone a jewel of the adult social consciousness; 154 of the girls and 104 of the boys want that very quality in their own associates—'a chum that won't desert you,' as a lad of nine years stated it, while a girl of 11 wanted something more than 'a fair-weather friend.' Selfishness is abominated by 99 of the children—62 girls and 37 boys. An affectionate, loving chum is desired by 52 girls and 29 boys; and a bright, talented chum by 38 girls and 20 boys. Modesty—a chum that is not bold—is esteemed worthy of mention by 44 girls and 20 boys, and obedience by 34 girls and 19 boys. Bravery, like size, fondness for play and abhorrence of profanity, tobacco and liquors, is masculine; 25 of the boys and 19 of the girls want a chum 'that is not a coward.'

Religion is one of the least important characteristics of these papers; 23 girls and 5 boys mention attributes that might be called religious. One wants a chum that is a Christian, another wants a chum that is a member of a certain church, and another specifies that his chum must attend church and Sunday-school and the society of Christian Endeavor regularly.

A 'respectable' chum is desired by 99 girls and 66 boys, and a 'nice' chum by 80 girls and 29 boys. Both these terms seem to the writer more or less indefinite and intended to include a variety of characteristics. 'Nice' was used almost exclusively by the younger children.

In spite of the fact that money is exalted to undue importance in the minds of American children, but 7 boys and 3 girls expressed a desire for a wealthy chum.

One girl wants a 'tough' for a chum; a boy of nine wants a policeman; a boy of eleven a negro; a girl of twelve wants her opposite—not what she is but what she would like to be.

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'THE REACTION-TIME OF COUNTING.'

In the last number of this REVIEW Professor Warren writes in the introduction to his interesting experiments on the reaction-time of counting as follows:

"The problem of counting was taken up by Cattell (*Philos. Stud.*, 1885, III., 121). * * * * The largest number for which the right answers exceeded the wrong varied between five and eleven, according to the subject. The higher numbers, however, were only correctly counted by those who had made many

trials; this leads to the suggestion that the subject may have become familiar with the number of lines on each card in the course of his practice, and that he may have afterwards judged the number from the width of space occupied by the lines on the card—an inferential process again. On this account Cattell's results seem open to question, and it was important that they be repeated with such changes in method as would avoid this possible criticism. This was one object in the present investigation."

In my paper I state that from three to six lines varying with the individual could be correctly given, with which the results now obtained by Professor Warren agree. None of the observers was always right in his estimation with more than six lines, and only one observer guessed more often correctly than wrongly with as many as ten lines. In these experiments I think the observers, especially he who did so well, were helped by 'inference.' Four may be 'grasped' as two two's and eight as four two's. Over thirty letters may be correctly seen when suitably combined in words and sentences. I do not understand, however, why Professor Warren questions my experiments on the ground that the higher numbers were only correctly counted by those who had made many trials. I state explicitly "Die Uebung scheint auf die Genauigkeit der Schätzungen keinen Einfluss zu haben. Das zeigt die folgende kleine Tabelle," etc. The observer with the best record was right in 56 % of the cases with 10 lines and in 76 % of the cases with 7 lines, which is not surprising in view of the fact that he could give occasionally not only the number but the exact character and order of seven numerals. I do not think that the length of the card was an important factor, more especially in view of the circumstance that the observer required a much longer time to estimate the length of a line than the number of objects presented.

Professor Warren further writes:

"All the published experiments on recognition time having been made with the hand key, which gives decidedly shorter times than the mouth key here used, it is impossible to compare them directly with these results."

I have published numerous experiments (cf. Wundt's *Physiologische Psychologie*, II., p. 373, ff.) in which a mouth-key was used for experiments on recognition-time, and have compared the times with the hand and those with the organs of speech. Indeed I have made a few experiments exactly like Professor Warren's, measuring what he calls 'the reaction-time of counting.' The results will be found in *Mind*, Vol. XII., p. 74, 1887, and *Philos. Stud.*, Bd. IV., p. 250, 1887.

I call attention to these unimportant matters with some hesitation, as they in no way interfere with the thoroughness and value of Professor Warren's experiments.

J. McKeen Cattell.

PSYCHOLOGICAL LITERATURE.

The New Psychology. E. W. SCRIPTURE. (Contemp. Science Series.) London, W. Scott ; Imported by Chas. Scribner's Sons. 1897. Pp. xxiv + 500.

Dr. Scripture's aim is to put the general reader in touch with the methods and results of quantitative psychology. He does not attempt to cover the entire field of research, and carefully refrains from broaching any of the theoretic problems which recent investigation and analysis have served to reveal. Even within the sphere of exact measurement he fails to point out the relations between the separate problems discussed. On the other hand, his range of topics is large, and represents fairly well the work actually accomplished in the experimental line.

The opening chapters, on Methods (Part I.), are excellent. The author urges the importance of exactness in investigation. Considering the amount of slipshod work that is given to the world as experimental psychology, we can excuse a certain diffuseness and persistance on his part in dealing with this subject. He points out the many opportunities for error and improper induction, and makes a strong appeal for greater precision in conducting the experiments and for a stricter mathematical treatment of the results. The chapter on experimenting contains a striking example of this. Dr. Scripture compares the careless methods used in certain 'psychical researches' with the scientific treatment of the problem of thought transference by Hansen and Lehmann. The chapter on Statistics is perhaps the most open to criticism of any in this part of the work. Much that is given in this chapter seems to belong more properly under the head of Experimenting or Measurement. It would be better had the line been more carefully drawn between the methods of gaining data and the methods of treating the data after they have been obtained. The latter—the mathematical treatment and interpretation of results—are presented with great clearness and detail; several notes are added in the appendix explaining difficult points, and tables of values are given for the more important formulæ, with directions for their application.

In view of the existing diversity of opinion among psychologists regarding the arrangement of their subject-matter, it is perhaps unjust

to take our author to task for his classification of topics. The discussion of experimental problems, which constitutes the bulk of the volume, is divided into three main heads: Time, Energy and Space (Parts II. to IV.). The 'Fachman' will be struck at once by the extra-psychological character of this classification. And this is but one instance in many, where Dr. Scripture has subordinated the psychological to the physical. Whether the 'new psychology,' or even the experimental side of it, can ever attain a perfect development by modeling its structure upon that of physics, is for the future to determine. The result in the present instance is a decided lack of unity. We find reaction-time placed next to subjective time-estimate and rhythm, with the influence of time on memory following immediately after. In another place the discussion of movement, muscular and pressure sensations is separated from that of sound and color sensations by a chapter on feeling and æsthetics (the 'golden cut,' etc.). An effect of this treatment is to magnify particular results at the expense of general principles. Common factors, such as fatigue, attention and association, appear in only one or two connections, and their real reach is not apparent.

Again, the author's choice of material is not always free from objection. A number of topics are inserted which have but slight bearing upon the subject; for example, several pages are devoted to a description of the mechanism of the kinetoscope and vitascope, whose essential psychological principle has already been sufficiently illustrated by the stroboscope. Another aspect of this criticism may best be shown by an application of the statistical method. In the part on Time there are 74 separate references in foot-notes to authors and laboratory work; of these, 21 are to the work of the Yale laboratory and its head, 18 to work done at the Leipzig laboratory, and only 26 to work done at all other laboratories together, the remaining 9 being references to independent writers. Without wishing to disparage the importance and peculiar exactness of the work accomplished at New Haven, it may still be urged that a symmetrical study of the subject should allow more space to the work accomplished at Leipzig and some eight or ten other laboratories in this country. This one-sided treatment is somewhat atoned for in Part V., where the author gives an historical sketch of the science. These chapters furnish a good résumé of the growth of experimental psychology from the time of Fechner and his fore-runners to the present day.

Whatever may be said against Dr. Scripture's way of dealing with the subject, his book is certainly rich in the matter of particular

problems. To give an example. The chapter on Time of Sensation includes descriptions of apparatus and methods of treating the following topics: the exposure necessary to recognize colors, the latent time and lag of sensation, the rhythm of attention in sensation and memory, the fluctuation of perspective, the effect of intensity on the shortest exposure-time, the effect of size and distinctness on the exposure necessary for the recognition of letters and words, and the principles of the stroboscope, kinetoscope and vitascope—all this within the limits of 32 pages. The various phases of the reaction-time problem occupy the two succeeding chapters (50 pages), and so on. The number of special problems discussed in the course of the volume is so great that it would be useless to attempt to review the entire list.

The descriptions are concise, but clear, and the various means at command for measurement are well illustrated in connection with particular experiments. Many diagrams representing actual results are plotted out, tracings of records on smoked paper are reproduced, and specimen tables of values are given to show the application of different methods. Dr. Scripture insists on exactness above all else, and this appears not only in the preliminary discussion on Method, but throughout the work, where he explains how to conduct the experiments and how to handle the results. In the several parts of the book different elements are made prominent. The special aim in Part II. is to illustrate the *methods* of experimenting and measurement; in Part III. the *results* are emphasized, while in Part IV., under space-perception, both of these elements are subordinated to the discussion of underlying *principles*.

The popular reader will find the subject presented in a tangible form, and will gain a good general understanding of the nature of the science and the work already accomplished. The form of presentation is dignified; it does not lower the subject by appealing to the sensational tastes of the public, as is sometimes the case in popular writings on the 'new psychology.' One can readily excuse the absence of scientific analysis and lack of completeness in Dr. Scripture's book, in view of its 'scientific atmosphere' and wealth of material.

The typography is generally accurate, and this is especially noteworthy in a work so full of figures, symbols and diagrams. We notice that the X and Y axes are interchanged in Fig. 30, and that the decimal representing Certainty is variously stated on pages 22, 287 and 288; both are very excusable oversights.

While Dr. Scripture is so accurate and precise in his numerical and detail work, it is to be regretted that he has not been more careful

in his choice of title. Whatever one's individual opinion as to what constitute the most important achievements of the 'new psychology,' it is but fair to recognize that all the advance has not been on the mathematical side. Genetic psychology has surely a right to be classed under the same head, as well as hypnotic and abnormal psychology. In fairness to these branches it would have been preferable to select a more distinctive title for the work.

HOWARD C. WARREN.

PRINCETON.

Hypnotism and its Application to Practical Medicine. OTTO GEORG WETTERSTRAND, M.D. Translation with medical letters on hypno-suggestion, etc. Henrik G. Petersen, M.D. G. P. Putnam's Sons.; New York, 1897. Pp. 166. Price, $2.00.

The fundamental interest in this volume is the medical interest in the possibility of utilizing the methods of hypnotic suggestion in the treatment of disease. Such application is by no means so well established as to make superfluous the record of the experience of a judicious and careful practitioner. Dr. Wetterstrand has carried into the countries of the far North the methods that have been developed amidst the southern-tempered environment of Paris and Nancy, and seems to have applied these methods with undiminished success. He enrolls himself with the adherents of the Nancy school who regard the mental efficiency of the suggestion as the essential part of all hypnotic processes and look upon physical manipulations as secondary and unessential. As a selection from a physician's case-book these records are very promising. Like all fairly selected records they show failures as well as cures, relapses as well as permanent benefit. But the general percentage of improvement and cure is remarkably high and the proportion of persons susceptible to this form of treatment is still more remarkable. The author holds that, with few exceptions, everyone can be hypnotized, though with some the process must be repeated with endless patience to meet with success. With one lady the attempt was made seventy times before it was successful, but after that she was cured of a troublesome ailment of twenty years' standing.

While a fair representation of diseases of physical origin appears in this record, yet the majority of cases relates to diseases of nervous origin and of functional nature. Insomnias, neuralgias, paralyses, stuttering, blindness, hysteria, neurasthenia seem particularly adapted to suggestive methods of treatment. Non-nervous diseases are often ameliorated, but it is not claimed that the real seat of organic disorder

is reached by suggestion. Dr. Wetterstrand does not propose hypnotism as a panacea, nor as unfailing remedy in cases to which it is applicable. Like all treatment it must be judicially used to be effective; and the physician must always be prepared to meet cases that baffle his skill. It is claimed that in modern therapeutics, which treats patients rather than diseases, hypnotism has an important place. Any one who wishes to examine the nature and strength of the evidence for this position may be referred to Dr. Wetterstrand's able and judicious compilation.

Dr. Petersen's additions to the English edition of the work can hardly be said to add to its value. His remarks on the nature of the hypnotic state are neither systematic nor critical, and his random treatment of allied topics is frequently unscientific.

JOSEPH JASTROW.

Sleep: Its Physiology, Pathology, Hygiene and Psychology. MARIE DE MANACÉINE. London, Walter Scott, 1897. (Imported by Charles Scribner's Sons, New York.) Pp. vii + 341.

"The English edition of this work (already published in Russian and in French) has been enlarged and revised by the author. For various additions and references she is indebted to the Editor." This unambitious preface is followed by four chapters, each of which deals with an aspect of the subject as indicated in the title. It is a timely monograph. Various lines of modern research converge upon the problem of sleep, while sleep itself, as a normal condition, is the point of departure for many explorers of the obscure phenomena presented by hypnosis. The author has evidently taken pains in gathering data, partly from her own observation and experiment and partly from the rather scattered literature of the subject. The bibliographies appended to the different chapters are an excellent feature of the work, though in some instances one might wish that later editions of well-known publications (*e. g.*, by Spencer and Wundt) had been cited.

The facts concerning sleep are threaded upon the psycho-physiological theory. Other current explanations are weighed and found wanting by reason of the common defect that they neglect the fundamental and direct cause of sleep. None of them is subjected to very searching criticism; they simply give way to the formula—"Sleep is the resting-time of consciousness"—which, however, takes account, as occasion offers, of the facts upon which the more exclusive theories —chemical, histological or vaso-motor—are based. "During sleep nothing is asleep within us except the brain" and the brain sleeps only

"in so far as it is the anatomical basis of full consciousness" (pp. 32, 70). Again "if sleep represents the resting-time of consciousness, consciousness demands, above all, a considerable flow of blood to the head" (p. 84). For normal sleep, besides the interruption of consciousness and the presence of cerebral anæmia, "it is also necessary that there should be active nutrition of the tissues, and apparently also active blood formation" (p. 89). From these passages and from the general treatment of the subject, it would seem that the psycho-physiological theory aims chiefly at emphasizing the relations between consciousness and sleep without excluding other factors.

From this point of view pathological conditions may be explained. If insomnia is due to an overtaxed or disordered consciousness, excessive sleep in its various forms, narcolepsy, automatism and hypnosis, is found in subjects whose consciousness for one reason or another is enfeebled. Hygienic prescriptions, therefore, should be directed towards the restoration of normal conditions on the mental side, and the necessity of such procedure is well illustrated in the detailed and interesting account of the hypnagogic state. The psychic life of sleep is accounted for on the principle that in sleep each enters his own world. This implies, *first*, a large liberty for the subjective and involuntary life of the neuro-cerebral apparatus which is dependent upon traces of impressions received during the waking state; *second*, a revival of ativistic tendencies. Dreams are carefully studied both in their normal and in their pathological aspects; but the question as to whether complete absence of dreams be possible is left open. The main theoretical conclusion of this final chapter is a modification of the formula already cited: sleep now 'represents the repose of the individual consciousness.' In the attempt to establish this view there are, as the author freely admits, many lacunæ. Still, the principal merit of the work is the prominence given to the psychological interpretation of such facts as are now at the disposal of science.

E. A. Pace.

Catholic University.

L'Année psychologique. Alfred Binet. Vol. III. (1896). Paris, Schleicher Frères, 1897. Pp., 825.

A third volume of the Année lies before us, the smallest of the three in size, but none the less valuable on that account. It is divided, this time, into three parts instead of two; the first contains original articles; the second, analyses and reviews; and the third, a bibliographical table of the psychological literature for 1896. The latter is

a reprint of the Psychological Index compiled by Professor Warren and Dr. Farrand and published by the PSYCHOLOGICAL REVIEW. In the second part, one finds analyses and reviews of about 117 discussions in the following subjects: Histology, Anatomy, Physiology of the Nervous System; Visual, Auditory, Tactual and Other Sensations; Memory and the Association of Ideas; Attention, Perception, and Reasoning; Illusions and Hallucinations; Emotions; Movements; Language; Individual and Characteristic Psychology; Sleep, Dreams, and the Abnormal; Automatism; Animal Psychology; Instruments; Miscellany. Binet and Henri have written nearly all of the reviews; the name of Binet occurs 67 times and that of Henri 25 times. Patrizi, Gomperz and Scripture have each given analyses of some of their own work. The analyses, as a whole, are very interesting; one has here a digest of a good deal of literature, and the work has been, for the most part, carefully done. In a few places one has the feeling that the point of view and the method of some of the German authors are not fully appreciated, *e. g.*, the work of Mentz on the influence of different psychic conditions on the pulse and breathing. The point of view throughout the work is that of the Paris school of psychologists, the personality of Binet dominating; and just therein consists the chief value of the Année.

The bibliographical table occupies 137 pages, the analyses and reviews 356 pages, and the original articles 332 pages. Of the latter there are twelve, too many to be analyzed here; we can only mention a few points.

The Année opens with an interesting article by Th. Ribot on 'Abstraction Among the Emotions.' He does not mean the association of the emotions with visual or other symbols. "It is a process of isolating an essential, or accidental, mark in a group of objects, so that it becomes for us the substitute for the totality; we thus unify resemblances and eliminate differences." In passing through a foreign land (*e. g.*) one becomes acquainted, we say, with the spirit of the country; and this is not a picture of men and nature, but a 'condensed residue of the affective states experienced' in passing through the country. It is a general emotional characteristic and a résumé. The same is true of operas and the works of authors. The terms 'moral milieu' and 'moral atmosphere' connote a general effective state which is an abstract and representative of many such states; they are fundamental marks extracted from an ensemble; they are simplifications. Then follows a very interesting account of the aim of that modern school of æsthetic producers called the symbolists. They aim

to express, not ideas or images, but emotions; they do not describe or name things; they strive to recall to the reader the peculiar emotion with which he looks upon an object. Their method is one of suggestion, which proceeds by describing the general or particular effect which the object produces.

In all this the word is a sign, par excellence. But, for the symbolists, a word ought to express less of thought than of sentiment; it loses, therefore, its intellectual function and sustains a new adaptation. For this purpose, one of the chief methods is to associate words in such a way that they lose their precise sense and become mysterious. Another method is the employment of new words, and still another, to give to words an exclusively emotional value. The symbolists use words for sounds, making them the instruments which suggest and represent emotion.

The author considers that emotional abstractions do not rise above the plain of generic images or 'recepts' on the intellectual side. But, we should like to ask, does not this imply too sharp a separation between the intellectual and emotional elements in mental processes? Is there a pure emotion or a purely intellectual element? We think not, however much we may analyze and generalize concerning it. But in that case, we do not have a good reason for saying that there are two processes of abstraction and generalization, the one intellectual and the other emotional. And moreover, as the author has said, the method of the symbolists is simply one of suggestion; and one naturally asks what the connection is between the law of suggestion and the law of abstraction and generalization. It seems to us it would be too artificial to say that we have first an emotional abstraction, and that then certain visual or auditory signs get associated with the abstraction so as to act as a suggestion upon it.

The next article deals with 'Variations of the Capillary Pulse at Different Hours of the Day.' The authors are A. Binet and J. Courtier. Apparatus used: the plethysmograph with rubber tube of Hallion and Comte.[1] The inestimable advantage of this instrument is the opportunity which it affords for comparing different curves; one can elevate the apparatus and bring it back to the starting point many times without changing the form or amplitude of the capillary curve. Among the results on the first subject are the following: The influence of the midday and evening meals is very apparent in an acceleration of the pulse, an increase in its size, a lowering of the dicrotism, or rather a lengthening of the point of the pul-

[1] See *L'Année psychologique*, II., p. 88, 1896, and I., 296, 1895.

sation, an acceleration of the vascular reflex, and an elevation of the temperature of the hand and arm-pits. The effect is due, in the authors' opinion, not to nervous excitation, but to the process of nutrition. With this subject, the entire body tends to cool off when he is engaged in purely intellectual labor, whenever the last meal is some time passed. The first determination of the capillary pulse in the morning, made before the toilette and before breakfast, and the third one, made before dinner and during the period of intellectual labor, show the lowest temperature, the slowest and smallest pulse, etc. With another subject, it was shown that the capillary circulation does not depend, as is usually supposed, upon the temperature of the hand (the member examined during these experiments). In the case of a third subject, it was shown that both the arterial and the capillary pulse undergo the same changes under the influence of a meal, viz., cardiacal acceleration, augmentation of the amplitude of the pulse, and a lowering of dicrotism. Likewise, in the case of the carotidal pulse the same effects are noticeable. One is forced to the conclusion, the authors believe, that the change in dicrotism is never a local effect, but a phenomenon which repeats itself in other arteries, and one which is seated in the general circulation. This is the most important result of these experiments.

The same authors have also reported the next series of experiments—viz., those on 'The Effects of Muscular Work on the Capillary Circulation.' Authors found that, with some exceptions, the heart and respiration are not accelerated. The first group of experiments were carried out by the use of the dynamometer. 1. After taking the capillary pulse of a subject three different times, one lets him give a maximal pressure four or five times in succession on the dynamometer. The hand is then replaced in the apparatus and the capillary tracing is again taken three or four times. The capillary pulse is depressed by about one-fourth or one-half, the depression lasting a short time and then disappearing. 2. The right hand remains in the apparatus, while the subject fatigues himself by pressing the dynamometer with the left. Immediately after the pressure the capillary pulse may be either greater or smaller than it was before; the point is blunter, and the dicrotism has flattened out to an oblique position from above downward. The duration of the modification depends upon the duration of the muscular effort which produced it. The arterial pulse shows the same modifications as the capillary. 3. Experiments were also conducted to determine what the effect of local muscular effort on the capillary pulse *during* the effort is.

4. Painful beating of the hand. 5. A strong and painful electric current, passed through the forearm whose hand was in the apparatus, produced contractions. 6. The effect of fatiguing the right hand is a pulse like that of the left hand, *i. e.*, a pulse with a less accentuated dicrotism. 7. Experiments with general muscular exercise. Finally, the conditions under which the dicrotism is reduced are precisely those under which the heart accelerates the pulse; and, likewise, we have not observed any displacement of dicrotism, during local muscular effort, where there is no loss of breath and no cardiacal acceleration. Acceleration of the heart may, as Hallion and Comte suggest, have some influence over the descent of dicrotism.

The same authors have a third report on the 'Capillary Circulation,' this time 'as influenced by intellectual labor.' Much interesting evidence is advanced in favor of the conclusion that the circulatory systems of the head and body are independent of each other; that the body is not necessarily deprived of blood when the head is full, and vice versa. The authors find: 1. An augmentation of the volume of the brain during intellectual labor. 2. This augmentation does not occur before, but some time after, the beginning of the cerebral work; it is not a cause but an effect. 3. We are ignorant as to the mechanism by which this augmentation of the brain during intellectual labor is brought about. As results of the experiments on the capillary pulse, we give the authors' résumé. Short and intense mental labor produced, as phenomena of excitation, vaso-constriction and acceleration of the heart and breathing; as phenomena of depression, a softening of dicrotism amounting, in some cases, to effacement, and a retardation of both the heart and the respiration. During the phase of excitation, the depth of the act of respiration was lessened. Intense mental labor prolonged for many hours, on the other hand, with relative immobility of the body, produces a gradual retardation of the heart, and a diminution of the peripheral capillary circulation. The discussion closes with an enumeration of the resemblances and the differences between intellectual and physical labor in their effects upon the capillary circulation.

The fifth discussion of the Année is also written by A. Binet and J. Courtier, on the 'Influence of the Emotional Life on the Heart, the Respiration and the Capillary Circulation.' The authors believe that there is nothing characteristic of either joy or sadness about arterial tension, for it may be either strong or feeble in either of these two contrary states. If this is true, the James-Lange theory of the emotions cannot stand in its usual form, for it holds that joy is the

effect of a vaso-dilatation, and sadness, of a vaso-constriction. The real contrast is not between agreeable and unpleasant excitations; but between excitations and states of rest, physical and moral. In intellectual work respiration becomes more superficial, while in the emotions it becomes deeper. "All the emotions which we have provoked are, whatever may be their quality, excitants to the nervous system; they promote vaso-constrictions and accelerate the respiration and the heart." They have experimented with surprise, fear, physical pain, sensations and emotions of pleasure, and voluntary psychic emotions. The result does not necessarily apply to very strong emotions, to very prolonged (chronic) emotions, or to emotions which powerfully excite weak or predisposed individuals.

As to the influence of music on the respiration, the heart, and the capillary circulation, the authors summarize their results briefly as follows: 1. In the majority of individuals every emotion (in this field) produces a vaso-constriction, an acceleration of the heart and of respiration, and an augmentation of the size of the thoracic cavity. The effects are more marked as the emotion is more intense. 2. In some very rare cases a sensation of pain and an emotion of sadness have produced a very slight retardation of the heart. 3. It is possible, as the observation of one subject, in particular, has shown us, that the form of the capillary pulse should change with the quality of the emotions—a fact which may some day permit us to make a classification of the emotions according to their physiological effects upon the form of the pulse.

The sixth discussion, written by Binet and N. Vaschide, deals with 'The Influence of Intellectual Labor, of the Emotions, and of Physical Labor on the Pressure of the Blood.' The authors begin by rejecting most of the instruments and all the work which have been realized in the past. Most of the instruments are intended to measure the pressure necessary to stop the beating of an artery, this pressure being accepted as the blood-pressure. The authors prefer the sphygmomanometer of Mosso, which shows what changes in the tracing of the pulse, with any chosen pressure of the instrument, result from the physical, or psychic, condition of the subject. Discussion has three parts: Technique, is a very interesting discussion of the apparatus and its application to the present problem. The effects of intellectual labor, of talking vigorously for three minutes, of excitations of the senses, of physical exertion, of physical pain, and of joy and sadness, were all the same in kind, viz., increase of blood-pressure, with other results which vary from one case to another. Simi-

lar results from four other subjects, *i. e.*, from five subjects in all. The experiments with constant pressure produced results similar to those with increasing pressure. In all experiments with constant pressure, much depends upon the pressure chosen, a fact which has not been duly taken into consideration by many of the experimenters in this field.

'Investigation of the Early Recollections of Childhood,' by V. and C. Henri. This is a report of the answers to a list of questions which were published in a number of the leading psychological journals, and sent by the authors and their friends to different persons, since the fall of 1895. The total number of responses numbers 123. The earliest memories in fully two-thirds of these cases date from ages between 2 and 4 years; some go back as far as 6 and 8 months, while some recall nothing which transpired earlier than the seventh or eighth year. Such memories must impress the child strongly, *i. e.*, they must be important to its life; they have been, in many cases, repeatedly recalled to mind during childhood and early youth. These memories are mostly visual images, such as objects, colors, positions, forms; but persons are very poorly remembered as compared with things. The forms of persons are often remembered, but no details; even sex is frequently not recalled. Early memories are never located in time except by reference to things which occurred later. Early memories seem to be recalled by resemblance to or contiguity with present experiences.

The eighth discussion is by N. Vaschide, on 'The Localization of Recollections.' After a hasty historical sketch, intended to show that the work of others has been purely theoretical, the author proceeds to his own experiments, conducted chiefly on himself, and supplemented by control experiments on others. On 2,200 pieces of paper he wrote French words of two syllables, some known and some unknown to the subjects. These he placed in three bags: in the first, known words; in the second, unknown; and in the third a mixture of known and unknown. He either drew out, or let some one else draw out, of the bags, 8, 10, 12 or 20 pieces of paper, which he either read one after the other, or had read to him; turned them over face downward, and arranged them on the table in the order of their drawing. He next wrote in a column all the words which he could remember, in the order in which they were drawn. This column was then covered; and, taking another strip of paper, he wrote on it a word which had been pronounced to him, and gave it a number denoting its place in the series of words drawn out. The written results

were compared with the original slips of paper which had been arranged on the table in the order in which they were drawn. Author finds six classes of localization, as follows: 1. Direct and immediate localization. 2. Localization by association in the usual sense. 3. Mediate localization without association; this characterizes the recollection of space in a series of words, where one or more words, which are, however, forgotten, belong. 4. Localization by association with a feeling; a group of words is 'painful,' has a sense of effort associated with it as a whole, and the place of any given word in the group is recollected with reference to the entire group. 5. Localization by reminiscence; subject has a vague impression that a word occupies a certain place, but does not know why. 6. Localization by reasoning; one has a vacancy, left in memory, and an unlocated word, after all the other words have been placed, and puts the word into the vacancy simply because the place is unfilled. But there are one or two points where these experiments do not satisfy us completely. In regard to the method employed, it is well known that the memory of words heard should be kept distinct from the memory of words seem; some persons remember visual, and others auditory, images best. It is not likely that the control experiments made by reading the words to a friend were at all comparable with those made upon himself by the author by reading over the slips of paper which he had drawn. It seems as though the experiments made by reading the slips ought to be distinguished from those in which the words were read to the author by a friend. Moreover, what sort of a localization shall we call it? The localization involved in writing the list of words—is it the same as the localization involved in giving to a word a number which denotes its place in a series to which one has listened, or in one which one has read? Again, the mere statement that localizations are not essentially memory-processes, but depend, to some extent, upon reasoning (as if reasoning itself were not dependent upon memory) does not seem to us to throw much light. It is hard to see the difference between the author's first two classes. By direct localization he designates the recollection that a certain word or words stand at the beginning, at the end, and in the middle of the series; but is not this localization by association? The same is true of his 'Mediate localization without association.' May not the 'Localization by reminiscence' be simply a case of 'lapsed links'? And, finally, is the localization by reasoning, properly speaking, localization at all? Both the method and the results of this piece of work seem to us to be confused.

The next discussion is by Victor Henri, entitled 'New Researches on the Localization of Tactile Sensation—The Experiment of Aristotle.' This is a continuation of experiments reported on the same subject last year. Like nearly everything which the author writes, the article is helpful in its allusions to the writings of others. All of these experiments were made with the fingers of the subject crossed as in the experiment of Aristotle. 1. The first result is as follows: "If one touches the fleshy parts of two crossed fingers with the two points of a compass, the more the points are separated, the one from the other, the more they appear to be drawn together." 2. Localization of the points touched, on a model of the fingers. The author rejects the historic theories of association and of local signs, as explanations of the phenomena. He considers them as illustrations of the tendency to assimilate present experiences to our habitual past experiences. This result seems to be in harmony with the best results of other investigators in this field.

'Study of Mental and Physical Labor,' by V. Henri. The author gives a résumé and criticism (1) of the methods and (2) of the results of investigations in this field, adding a complete bibliography of the subject at the close. Four factors enter into labor of any kind, viz., exercise, fatigue, mental tone or cheerfulness, and enthusiasm. Author points out many rules of pedagogical interest in regard to the use of both mind and body. The article will be found useful to all who are taking up the subject from the beginning; it is original chiefly in its suggestions, its criticisms, and its treatment of the work of others.

In his 'Reflections on the Paradox of Diderot,' M. Binet gathers together a number of declarations from prominent actors and actresses in regard to the question whether an actor properly feels the emotions he portrays. Diderot thought not; the essence of his arguments being the plea that a man cannot do more than one thing at a time; he cannot recall his part, criticize himself as he plays, and at the same time really live the emotion he is simulating. It is the almost universal testimony of actors and actresses in regard to the question that they *do* feel the emotions they portray. It is only so long as the actor's incarnation of a personage is incomplete that he is double.

But we should like to ask what law of our mental life it is which enables the actor to incarnate another personage in himself, and the spectator equally to incarnate a personage, by submitting himself to the deception, which he knows to be a deception, that he is viewing reality. In what sense is the actor original in his playing? To what

extent does he merely copy? And, is there any such a thing as *mere* copying? The author remarks that each actor must interpret his rôle according to his own peculiar susceptibility; but just what is the psychologist to understand by this?

The last article is by M. Binet; it is entitled, 'Individual Psychology—The Description of an Object.' The author thinks that individuals differ more in the higher processes, such as conception, memory, reasoning, etc., than they do in the simple processes of sensation and movement. Hence he aims, in this study, to place a number of persons before the same object, in order to see what differences in psychic processes the object will awaken in the different person. 1. Experiments with school-children in Paris and Versailles. Photographs of a painting are held before the children for 2 minutes; they then proceed to describe, within ten minutes, what they saw in the photographs. The author remarks that the memory of such a picture is no doubt reducible to the memories of certain lines, colors, forms, surfaces, ornaments, etc.; and his experiment is rather one in ideas, as compared with these. As results, he found, 1. A simplification in the number of objects; instead of 22, they mention only about 8. 2. The selection of objects, both in the case of the attention and in that of the memory, follows a definite rule which is the same in both cases: the selection is based upon the importance of the objects; above all, upon the importance of the part which they play in the picture. Persons, more than objects, fix the attention. Memory selects more than the attention. The picture is an illustration of a fable by La Fontaine, 'The Laborer and His Children,' and the children, who all know the fable, undergo, to some extent, illusions of memory; they wander away from the picture to details and circumstances which they remember from the story.

A study of the individual differences between the descriptions handed in by the children shows certain types of mind, which Binet names the descriptive, the observing, the emotional and the learned. The first merely see, in the picture, an ensemble of objects without being impressed with the event which they portray. The second type give their attention, not to the objects alone, but to the part which each object plays in the whole scene. The third type fill their description with sentiment, and neglect details and facts. The fourth give a résumé of the story of La Fontaine. In similar experiments with older persons, the author had a similar experience. Here again he finds four classes, which he names the descriptive, the observing, the erudite and the imaginative or poetical. These types are

accounted for by the dominance of some one or other of the mental faculties over the remainder. This dominance he names mental orientation (*l'orientation d'esprit*).

Looking at these discussions as a whole, one cannot help feeling that both the method employed and the results obtained are sometimes not quite satisfactory, and one wishes, even in other cases, that the results had been worked over more thoroughly. But it seems to us that the Année is both interesting and valuable; it suggests a great deal of work yet to be done, in addition to the valuable results which it actually conveys; and this is one of the great services of any book. Excepting the discussion of Ribot, probably the best work in this number is contained in the articles on the capillary pulse, the heart and the respiration, and these are as interesting from a physiological as from a psychological point of view.

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THE QUESTIONNAIRES FROM CLARK UNIVERSITY.

A Study of Fears. G. STANLEY HALL. Am. Journal of Psychology, VIII., 147–249. Jan., 1897.

The chief aim of this article is to emphasize the influence of ancestral experience and selection in the formation of the multitudinous fears of childhood and youth. These fears are taken to be in part true instincts and an effort is made to show their origin. So with fears of falling, of high places, of confinement, of water, the heavenly bodies, darkness, shock, various animals, of eyes, teeth, fur and of strangers. Fears of thunder and of ghosts are referred back to the times of primitive religion.

While all ought to agree that this hypothesis is a proper one to test, it may well be claimed that the author has in its service misused the facts from his questionnaire replies. In his discussion of the primeval experience of hovering, he gives no clear reason why falling should be instinctively *feared*, and does not notice that fear *when* falling, fear *of* falling, and objectless fear when on high places, are three quite different things. Again, an instinctive fear of water seems less probable than its direct opposite, an instinctive ability to swim. To attribute fears of sky, cloud, etc., to 'echoes of the grander and more awful phenomena of primeval weather' is certainly a strain. Finally the fear of teeth ought, on this basis, to be more prevalent than

it appears to be. Moreover there is nothing in the extent of these human fears to match the universality of a genuine animal instinct. Children do not fear thunder or snakes as mice do a cat. Some carefully observed cases show no fear at all. Thus those previously inclined to leave all to individual experience and imitation may well fail to see Dr. Hall's conclusions in his data.

A few additional considerations may prevent such a rejection. Distaste at confinement, fear at shock and of large strange objects, are so common amongst animals and so prevalent in very young stages of human life that they are presumably instinctive. Yet they figure very little in the questionnaire replies. So the tendency of the questionnaire may have been really to discourage the reporting of instinctive cases. Further, very many instinctive fears (if there be such) are undoubtedly temporary and may pass without any chance for manifestation and record. Dr. Hall's evidence also suffers in quality because of the too great age of the subjects.

More fruitful than a host of questionnaires would be, I should think, a set of experiments on children from birth on, in circumstances where their experience of and instruction about things could be regulated and its influence as a disturbing factor completely known. Then an instinctive act would be unmistakable.

The Psychology of Tickling, Laughing and the Comic. G. STANLEY HALL and ARTHUR ALLIN. Am. Jour. of Psychology, IX., 14–2. October, 1897.

In discussing ticklishness the authors invoke, to help explain the sensitiveness to minimal touch, the early psychic life when touch was the only sense. It thus represents 'the very oldest stratum of psychic life.' Another factor in developing ticklishness is said to be experience with parasites. The different degrees of ticklishness in different parts of the body point to its origin from 'gestures of danger without any strong sense of it.' Finally minimal contact is classed as the trace 'of the first pleasures not directly due to food or sex, with which psychic development began.' One finds it hard to get any harmonious theory out of these several notions, and the declaration that reactions to *slightly* noxious stimuli are more likely to be inherited than reactions to stimuli which injure an animal enough to decrease its vigor and reproductive efficiency, is a rash one to make in the face of present-day views of heredity.

That children so often laugh at animals is, in connection with the facts of imitation, referred to their being nearer 'the parting of the

ways' into all the specializations of animal life. The pleasure involved is caused by the functioning of rudimentary psychic organs. This 'parting of the ways' theory seems rather wilful and far-fetched. Less so, perhaps, is the suggestion that laughter at recovery from slight fear is due to the stimulation of rudimentary organs, which stimulation causes their atrophy. However, the whole theory that psychic organs are led to disappear by being stimulated runs so contrary to the general law of habit that it is a weak support for any explanation.

Besides these and other suggestive theories, the paper contains a large amount of valuable data concerning the physical act of laughter and its many causes. The beginning made to collect accurate measurements of the appearance and sounds of laughter by the camera and phonograph (to which the writers refer) is most praiseworthy.

A Study of Puzzles with Special Reference to the Psychology of Mental Adaptation. E. H. LINDLEY. Am. Jour. of Psychology, VIII., 421–493. July, 1897.

The phenomena of puzzle-interest and puzzle-solving are used to throw light on the nature and usefulness of mental play, and on the comparative 'natural logic' of men, children and animals. To this end the author presents data on the interest and different sorts of puzzles, and at different periods, on the nature of the puzzle's charm, and on the methods of solving a unicursal puzzle by Tait. Following Dr. Groos he regards the mental play involved as a preparation for the rational life. In particular, puzzle-play 'may develop mental flexibility, versatility, and even powers of will.' The different ways of solving Tait's puzzle used by children of different ages show a progressive development from animal-like trial and error to conceptual analysis.

A Study in Moral Education. J. R. STREET. The Pedagogical Seminary, V., 5–40. July, 1897.

Mr. Street presents the results of his examination of a considerable number of replies to a questionnaire about various influences on the moral nature. Among these were rewards, punishments, teachers, companions, parents, games, books, adages, etc. The aim of the article is chiefly practical and no important new theoretical conclusions are drawn from its data. The advice concerning moral training which the author gives is such as would be generally acceptable, apart from what authority the facts from his report may give to it.

Imitation, heredity, the law of habit must all be taken into account, he says, and the higher emotions and tastes must be cultivated. One good suggestion is that boys may lack in boyish and manly virtues from the absence of male teachers during their childhood.

Some Mental Automatisms. E. H. LINDLEY and G. E. PARTRIDGE. The Pedagogical Seminary, V., 41–60. July, 1897.

In response to a questionnaire a number of people reported that they were subject to mental automatisms in the form of counting, avoiding, grouping or bisecting, the percentage being 81.3, 77.1, 66.3 and 40.6 respectively. Selected answers and an enumeration of the objects counted, avoided, etc., are given. An effort is made to explain avoiding as the effect of ancestral experiences when the foot claimed a large share of conscious attention, while the other automatisms are referred to early acquired experience, such as counting and drawing at school. A nervous counterpart is found in a *passé* set of functions, a submerged tract, which becomes active only in such favorable circumstances as distraction or weakness. The hypothesis of Duval and Demoor that the dendrites and neurites are contractile is appealed to in support of this view, though any other theory of brain action would be quite reconcilable to it.

One fact which seems important to the reviewer has not been dwelt on by the authors, namely, that those who have one automatism frequently have others, as the percentages show. This leads one to the opinion that the really useful matter is to understand, not in what particular direction heredity or experience shall cause the automatism to display itself, but what characteristic it is that makes a certain type of mind so liable to these automatic performances. Surely not those of us, who in childhood did most counting, are most liable to automatic counting. Nor do the avoiders come of ancestry especially drilled in careful walking or climbing. But from the facts of pronounced mental pathology, one would expect in these so-called normal cases some common feature which makes soil fit for automatisms to grow. To discover and define this would be of considerable practical as well as theoretic importance.

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VISION.

Ueber die Bedeutung der Convergenz- und Accommodations bewegungen für die Tiefenwahrnehmung. M. ARRER. Philosophische Studien, XIII., 116–161, 222–304. 1896–7.

The controversy as to whether sensations from eye-muscles render

essential aid in the formation of our visual perceptions of distance has led to much apparent experimental support of both possible positions. Arrer has repeated several of the more important methods of experiment with much care and thoroughness; has ably reviewed and criticised the argument on either side; and gives strong support to the view which regards the muscular sensations as important elements in these perceptions.

In all his methods of experiment conditions are so arranged as to exclude, so far as possible, all secondary criteria of judgment and to isolate convergence and accommodation. His main series repeats with improvements a method of Wundt. The object of fixation, regarded monocularly or binocularly through tubes, is a thread seen against a bright background. Two complete series of tests, by the method of minimal changes, were made on each of eight persons, to determine the fineness of their discriminative ability in judging distances. His results are largely a confirmation of those of Wundt; discrimination is somewhat less fine when the second position of the thread is further away from the eye than when it is nearer; the values for the former case gain relatively faster in fineness than for the latter as the thread is brought nearer; about the near point of accommodation both values approach one another; binocular measurements show a finer discrimination than is possible monocularly, and perhaps also less difference between the values for receding and approaching distances, the latter, however, remaining finer. It is shown that if the values measuring the fineness of discrimination in the binocular tests be compared with the distances for which they apply, a relative constant appears, varying for different observers from about $\frac{1}{30}$ to about $\frac{1}{60}$; and that the same relation holds between the angle of rotation corresponding to the just observable distance, and the degree of convergence. Wundt thinks that this validity of Weber's law points to the importance of eye-movements for judgments of distance; but Arrer does not regard it as entirely unambiguous. Again, in the monocular experiments, the following facts were established in regard to the criteria used in judging. Beyond the far point of accommodation, when such exists, the apparent thickness of the thread is the criterion on which the judgment is based. Between the near point of accommodation and the eye, the judgment is determined by increase or decrease in the clearness of the thread. Within the field of possible exact accommodation, however, the distinctness of the thread does not change, and the judgment for relative distance is much finer than that for the corresponding apparent difference in thickness of the thread. This fact

would seem to necessitate the assumption that the muscular changes accompanying accommodation, assisted perhaps by movements of convergence induced in the unused eye, furnish the criteria for the judgment. But inasmuch as the absolute exclusion of all secondary criteria by this method of experiment may be brought in question, Arrer tested also two other methods. One of these was that of Wundt, where both threads, whose distance is to be compared, were presented simultaneously. The monocular results differ little from those of the previous method; binocularly, the just perceivable differences are almost too fine for measurement.

Hillebrand, and later Dixon, with some improvements in method (see this REVIEW, I., 540, and II., 417), attempted to solve the problem by experiments in which the object of fixation was the edge between a movable black screen and a brilliantly lighted background, the comparison being made both for a gradual alteration in position of the screen, and for two positions of which one was suddenly substituted for the other. Arrer repeated these experiments, and all three observers found little or no ability to judge distances accurately. To Hillebrand it seems evident that muscle-sensations from accommodation and convergence are thus shown to have no importance for the perception of depth, the true criterion being furnished by the circles of diffusion and their correction through consciously willed alterations in accommodation. To Dixon the main criterion seems to lie in the difference in ease and rapidity with which accommodation alters itself to fit the changed positions. Arrer, however, criticises this method of experiment, and finds in its results no evidence against the importance of muscle-sensations for reasons some of which are given below.

From an examination of the methods of procedure and the subjective impressions of the subjects who took part in his experiments, Arrer shows that the observer cannot estimate relative differences in distance without forming an idea of the absolute distance of the object; and all variations of the latter are of important influence on the former. The second method of experiment, in which the two distances were simultaneously presented, is a further proof of the fact; for observers who failed to make correct judgments by the first method succeeded with the second, because the latter does not involve the more or less difficult retention of the memory of the absolute distance first presented. Whether this idea of absolute distance is *correct* or not does not matter, if only it is *definite* and uniform under like conditions. This the writer considers himself justified in assuming, and discusses at length the various theories as to its subjective causes.

Hering's theory of pure unanalyzable sensations of depth, yielded in the absence of empirical data, by our consciousness of the movements we will to execute, is shown to involve insuperable difficulties. These difficulties, however, are shown to disappear under the view attributing the definite distance-localization to sensations from eye-muscles; this view is applied to the full explanation of the results given by the first method of experiment and is shown to accord with subjective observations, and the many objections that have been raised against it are given satisfactory answers. The results of Hillebrand's method alone seem to oppose it. But the writer points out serious defects in the method, among them the irradiation due to the strong contrast between the two halves of the field. He shows also that the subjective procedure cannot be such as Hillebrand describes and that his theory is therefore inapplicable. But his chief contention is that a mathematical line, like the limiting edge between two contrasting fields, cannot be given a definite distance-localization, without which judgments of relative distance are impossible. Only by the aid of secondary criteria could the experiment succeed. A perception of distance is not composed simply of eye-muscle sensations alone. There must be also an *object* to localize, with whose perceived content these sensations may fuse. When so fused with other sensory content into a true distance-perception, they furnish within that perception the measure whereby different perceptions of distance can be compared. No other elements can furnish this measure, for under the various conditions of judging every one of them can be shown to be dispensable. Only convergence and accommodation sensations remain constant in all varieties of experiment; and they fail to give true criteria for judging distance only under circumstances where the absence of all other material makes the formation of a perception of distance impossible, for which they could serve as the measure.

Beitrag zur experimentellen Bestimmung geometrischoptischer Täuschungen. E. BURMESTER. Zeitschr. f. Psych. u. Physiol. d. Sinnesorg., XII., 355–394, 1896.

Burmester made measurements of the degree of illusion under various conditions in figures of three types, viz.: The Poggendorff figure, in which two parallel lines (or a wide solid stripe or line) are cut diagonally by a straight line, the latter being interrupted in the space between the parallels; the 'Schenkelfigur,' in which a straight line is drawn at an acute angle to one of two parallels, and from the point where its imaginary continuation cuts the other parallel a second

transverse is drawn back and outward at the same angle; and the 'Streckenfigur,' consisting of a simple straight line interrupted through a part of its extent, thus being identical with the Poggendorff figure with the parallels omitted. In the two first-named figures the point of departure of the second transversal appears to be displaced more or less above or below the point where the second parallel would be cut by the prolongation of the first transversal. In the 'Schenkelfigur' the amount of this apparent displacement is twice that of the Poggendorff figure. In both the amount of apparent displacement (v) is proportional to the distance between the parallels (u) and to the cotangent of the angle of inclination (w). In other words, if k is a constant varying with the individual, $v = ku \cot gw$. The illusion shows a constant diminution with increasing length of transversals. The apparent displacement in the 'Streckenfigur' is in a direction opposite to that of the Poggendorff figure; it is very slight, and is subject to large variations in estimation. It can be neutralized by a proper combination with the Poggendorff figure.

Quantitative Untersuchungen über die Zöllnersche und die Loebsche Täuschung. G. HEYMANS. Zeitschr. f. Psych. u. Physiol. d. Sinnesorg., XIV., 101–139, 1897.

In Zöllner's figure the intensity of the illusion attains a maximum when the angle of inclination is 30°, from which point it decreases on both sides; and it increases regularly both with increase in length of the transversals and with decrease in the distance between them. The illusion can be explained by the assumption that "the vivid idea of a movement in a particular direction arouses the appearance of a movement in the opposite direction, which adds itself algebraically to any movement that may be actually taking place." If the eye follows along the main line, the transversals constantly attract the attention, produce a tendency varying with their varying distance to diverge in their direction, and hence make the main line appear to move and therefore to decline in the opposite direction. The angles between main and transversal lines are of secondary importance, their varying distance from one another of prime importance. This can be shown by figures in which no angles are made; and of these a number of new ones are given.

Loeb's illusion demands this explanation, and is of the same essential nature as that of Zöllner. For measurement, it is best shown thus: Three parallels are arranged, the middle one equidistant from the other two. A disturbing line is now placed near one of the

outside parallels. The latter will appear displaced toward the two others, and the position of the middle one must be changed if it is still to appear equidistant from its neighbors. The degree of illusion increases at first with the distance between main and disturbing lines, reaches a maximum, then rapidly decreases. Now in the Zöllner figure, a transversal may be regarded as such a disturbing line whose influence varies as its distance varies and which thus produces the apparent inclination of the main line. But if so, then, after measuring the intensity of the Loeb illusion for different distances, it should be possible to determine beforehand the degree of apparent inclination in a given Zöllner figure. This Heymans successfully accomplished, and hence finds support for his claim that the two illusions are essentially identical. Every explanation which applies to the one and not to the other must therefore be rejected; and this is true of theories of Lipps, of Hering, of Guye, and of Thiéry. The view of Helmholtz and of Loeb that these are phenomena of contrast may well be accepted; but the more intimate nature of contrast itself cannot yet be asserted.

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Il calculatore mentale 'Zaneboni.' GUICCIARDI and FERRARI. Reggio-Emilia, Tipog. S. Calderini e Figlio, 1897. Pp. 52, with portrait. (Repr. fr. Riv. Sper. di Freniatria, XXIII., fasc. 1, 2.)

The subject of this monograph is an Italian who has recently come into public notice as a rapid calculator. Signor Zaneboni's special power consists in his extraordinary memory for numbers. He has memorized the population of a list of 227 cities, the distances and fares between all points on the Italian railroads, the number of hours, minutes and seconds in a year, a large number of natural squares, cubes and higher powers, etc. His public exhibitions consist largely in answers to questions which ring the changes on these data. He has learned the products of most numbers of two figures each, can add, subtract and divide quickly, and extracts roots, finds powers up to the fifth, and performs many other complicated operations with remarkable readiness.

Z. is a man of 28 years. His family history shows no psychiatrical or neurasthenic antecedents; some of his relatives are endowed with a memory above the average. His temperament is buoyant, but he is extremely sensitive; he is of a cold disposition sexually. The authors were allowed to subject him to a large number of psychologi-

cal and psycho-physical tests, which they modeled after those of Toulouse upon Zola, and of Binet upon Inaudi and Diamandi; in some cases the identical tests were used. The completeness of the series was somewhat impaired by the subject's unwillingness (or inability) to perform the required operations with sufficient care and exactness when they did not involve numbers directly; in such cases it was found impossible to keep his attention long fixed on the task.

The psychological tests showed a marked contrast between the subject's *acquisitive* and *conservative* memory. The former was found to be rather slow: Z. required 6 minutes and 45 seconds to memorize a series of 25 figures, and the next day was able to remember only the first 4 correctly in order. On the other hand, numbers learned automatically, such as those which he used in his exhibitions, were held in mind (without any mnemonic devices) with remarkable tenacity. The subject was of a distinctly visual type, with the kinæsthetic memory also well developed; his auditory and speech memories were poor. The area of perception, tested by means of an electric flash, was found to be about the average, in the case of letters (3–4); for figures it was extraordinarily great (8).

Z.'s reaction times were 75.6σ for auditory stimuli, 129.7σ for tactile, and 205.6σ for visual; these results were the mean of five reactions each, with no preliminary practice or instruction as to the direction of attention. For the visual reactions the mean variation was only 5.3σ. Tests of association (with the same list of words as that used by Toulouse on Emile Zola) gave 35 visual, 1 temperature, 1 gustatory-olfactory and 4 verbal associations out of 50; the remaining words brought out no association whatever. (Zaneboni, as the authors remark, is a man of rather limited education.) The test of attention was the subject's rapidity in canceling one letter, wherever it occurred, in a series of words, and one figure in a series of numbers. Z. canceled at the rate of 1 letter per second and 3 figures in 2 seconds. In testing the rapidity of addition, subtraction, multiplication and division, the identical numbers were employed which Binet used with Inaudi. Comparing the two, Z. is found to be always longer; the difference is very slight in the simpler tests, but increases rapidly with the complexity of the operation. When Z. explained his method of procedure, it appeared that his facility in the work was due to memory rather than calculation; in multiplying, *e. g.*, he would first separate his two numbers into parts whose products he already knew, and then combine the results by addition or subtraction. The *ensemble* of tests shows Z. to have a mind rather limited in general, but

possessed of a single prominent faculty, which he has developed enormously and to the detriment of the rest.

The authors compare their subject and others of this type with persons of average mental development, with a view to determining the general effects of an unusual development of one phase of memory. They are of the opinion that this kind of development is a decided detriment to mental progress. Such men as the famous calculators are geniuses indeed, but they are geniuses of a low rank; their memory is of a sort in which association by contiguity predominates, and the higher types of association remain undeveloped in them. They are of no value to the world except as curiosities; in every known instance, the applications of their powers have been merely puerile.

In closing, the authors emphasize the need of distinguishing between the several 'partial memories.' Their results give full support to the view that the memory must be treated as a collection of semi-independent functions, rather than as a single faculty.

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Experimentelle Untersuchungen über des Zusammenwirken des akustisch-motorischen und des visuellen Gedächtnisses. JONAS COHN. Zeitschrift f. Psychologie u. Physiologie der Sinnesorgane, XV., 3, pp. 161–183.

The aim of the experiments recorded in this paper was to ascertain what part is played in memory by the visual, auditory and motor elements which coöperate in the ordinary exercise of this function. Letters from the ordinary alphabet were taken as material for the experiments: twelve consonants arranged in three rows were presented in each case to the subject. The variations in memory were brought about by requesting the subject in one group of experiments to read the letters aloud, in another to repress articulatory movements while learning, and in three other groups to pronounce a series of vowels or numbers while reading the consonants. The result was ascertained at first by testing the memory for particular letters, but later by allowing the subject to reproduce all that he could remember; in both cases there was a pause of ten seconds between memorizing and reproduction, which the subject filled up by repeating the numbers one to twenty. There were thirteen subjects engaged in the research: the number of experiments with a particular modification of memory furnished by a given individual varied from twelve to fifty. The results are analyzed not merely with regard to correct and incorrect answers,

but also with reference to the different kind of confusion and disorder, such as those in regard to the shape and position of letters. The numerical analysis of errors is, at the same time, brought into relation with data derived from a general description of the type of idea and memory of the individual subjects.

Two conclusions are brought forward by the author as receiving experimental proof in this research. 1. Where memory is based chiefly on auditory-motor images it is more extensively interfered with by a disturbance of an auditory-motor character than a visual memory would be. 2. When the disturbing elements have an auditory-motor character, an increased use is made of the visual factors in memory wherever possible. The analysis of the special forms of error offers interesting facts and suggestions, but the number of experiments is too small to admit of secure generalization on such detailed points.

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Psychische Präsenzzeit. Von L. WILLIAM STERN. Zeitschrift für Psych. u. Phys. der Sinnesorgane, XIII., 325.

In his discussion of 'Psychische Präsenzzeit,' Stern shows clearly and forcibly that the psychological unity of a content of consciousness is what he calls the unity of its *Auffassung*, and that accordingly, whatever is actually apprehended as one, no matter how diverse the elements into which it may be analyzed, is psychically one. The analysis, as he says very acutely (p. 329, Note), is a 'later abstraction;' its "elements are not discovered again, but are first produced through the analysis." Stern goes on to show that a succession, though objectively a multiple, is yet often a psychological unit because taken as one. The actual duration of the 'act of consciousness,' whose content is regarded as a unit, is called *Präsenzzeit* (p. 327). This is an admirable statement if regarded as a definition of the objective duration of the fact of consciousness which 'appears single.' It is not, however, a psychical analysis of what one means by 'present,' and, in fact, is later supplemented by the definition of the present as the immediacy of the percept, in so far as we are conscious of it ("Werden Wir uns der Unmittelbarkeit der Wahrnehmung bewusst, so ist ihr objekt uns gegenwärtig"). This part of the discussion makes it very clear to the writer of this review that Stern is really considering not a psychic time-distinction at all, but rather 'persistence' or 'duration,' and 'oneness,' as elements of conscious contents. But succession, not duration, is the stuff of which the time-consciousness is

made, and the present can not be accurately defined without reference to its connection with past and with future. Of this 'inner connection,' however, Stern makes no account. He is correct in asserting the possibility of a direct consciousness of time against the arguments of those who confound 'feeling of succession' with 'succession of feelings,' but the 'objective ground' of such a direct consciousness is succession rather than duration (*Ausdehnung*).

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On the Time of Reflex Winking. DAVID P. MAYHEW. Journal of Exper. Medicine, II., 1, p. 35. January, 1897.

The stimulus was a slight blow from a small spring hammer upon the face about an inch below and to the outer side of the outer canthus of the eye. The fall of the hammer broke an electrical circuit which was again broken, at the wink, by the separation of a wire from a piece of platinum foil fastened to the eyelid with sticking plaster but insulated by a rubber membrane. Both breaks were recorded on a revolving drum along with a tuning-fork curve. Experiments were made on sixteen subjects, male and female. The average from 450 experiments was 0.0420 second. This is the mean total time: it was not deemed advisable to subtract the time taken by the transmission of the nerve impulse and the latent period of the muscle. Compared, however, with the least mean total obtained by Exner (0.0578 sec.) with electrical stimulation, the figure given above shows a lowering of the time for reflex winking. For different individuals the mean times varied between 0.0351 and 0.0491 second. Sex had no influence, nor was the record affected by natural winks occurring shortly before it was taken. To test the influence of the mind, the subject in some experiments was warned of the blow or required to add numbers or to press a manometer bulb and keep the mercury level. From the results thus obtained with nine persons, it would seem that apprehension shortens the time.

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The Psycho-Motor Problem. HERBERT NICHOLS. Amer. Jour. of Insanity, July, 1897.

This article constitutes one of six lectures delivered at Johns Hopkins University in March, 1896, on 'Modern Psychology and its Bearings.'

Dr. Nichols in a few words recalls the nature of the reflex and of automatic action of nerve cells, asserting that the older ideas as to the

automatic nature of protoplasm in general and of ganglion cells in particular are fast losing their ground. While he admits that protoplasm may have some power of self-originating movement, for the most part an outside stimulus is the sole determinant of action in organic matter.

He argues that since any kinæsthetic sensation may by proper association become directly motor, we need not doubt that sensations other than kinæsthetic, may, under the proper conditions, become directly motor, in short, that any and every sort of sensation may so act. The problem of motor ideas, therefore (except as regards instincts), is rather a discussion of the development of the reactions by experience and association than a research into the conditions of their anatomical necessity.

After discussing the parallelistic theory and the relations of 'mind and body' generally, he comes to the conclusion that all mental states whatsoever are motor with equal functional directness, though not with equal functional strength.

The motor problem seems to Dr. Nichols to stand at present on these five statements: First, all sensations and mental states are motor. Second, The entire neuro-muscular organism acts primarily as a whole. Third, The laws of association are universal for the entire nervous organism, motor as well as mental. Fourth, Our instinctive capabilities, multiplied and complicated in their development in accord with these laws of association, abundantly explain the definite rise of psycho-motor habits, and the variety of reactions which constitute psycho-motor phenomena. Fifth, 'Inheritance,' 'Education,' 'Opportunity,' 'Environment,' individual and social, are the watchwords for future Economy and for future Religion.

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EMOTION.

The Nature of Emotion. DAVID IRONS. Philosophical Review, VI., 242–256, 471–496, May and September, 1897.

The Primary Emotions. DAVID IRONS. Philosophical Review, VI., 626–645, November, 1897.

In the first paper directly, and in the second in connection with a criticism of other views, the author develops the thesis that emotion is an aspect of mind ultimate, primary and unanalyzable. It is a 'feeling attitude' distinct from cognition, pleasure-pain and conation and having its own special conditions and effects. It is feeling, and so

subjective, but as 'centrally initiated reaction' not, like pleasure-pain, mere subjectivity; it is attitude, and so has 'objective reference,' but a different objective reference from that of cognition. Impulse and volition are its consequences, a certain cognition is its antecedent, pleasure-pain is a concomitant. The theories criticized are, that emotion is pleasure-pain (Horwicz); that it is conation (Godfernaux); the James theory in various forms; the theory that it is excitement (Herbartians); the theory that it is pleasure-pain *plus* cognition (*e. g.*, Sully and, with modifications, Wundt). The author repeats his objections to the James theory with his accustomed vigor, but without adding anything essentially new to the criticisms he has published already. As regards the main question, he is certainly right in upholding the uniqueness of the emotional attitude and, so far as current theories deny or seem to deny it this character, in insisting thereon. James, at any rate, has admitted it and much besides in this REVIEW (I., p. 521). Emotion is not cognition, conation, pleasure or pain, nor any conceivable mixture or fusion of them. But does it follow that it cannot be analyzed? No more than that the living body cannot be divided into limbs, head, trunk, etc., because the organism is a unique synthesis of parts and functions and not the mere sum of them nor a fusion in which they lose their identity. We proverbially murder to dissect. Similarly the synthetic unity (as we hold) of the functional attitude in emotion cannot be constructed out of the elements said to be discoverable in the concrete experience itself, nor can it be pointed out as one element among or above others in the content. It is unique. The dispute then seems to turn on the nature of psychological analysis. Dr. Irons appears to hold that the emotional attitude is not analyzed when the sensations, ideas, feelings, impulses are named and their relations described essential to the experience; other psychologists hold that it is. According to him these factors are causes, effects, concomitants of the emotion, not its constituents. On this view emotion exists, not as the unity of a complete process in and through which an experience is had, but rather as an indivisible point for other contents to attach to. The difference between Dr. Irons and his opponents seems to be this: he is interested in logically defining the emotional attitude in the abstract as function, they are interested in discovering the constituents of content in the experience of it. That visceral changes are not essential to such constitution, as Dr. Irons contends, hardly seems to be made out. It is denied, indeed, that any commotion at all is necessary. "Hate," it is said, "may be very intense, while the general mental condition is free from excitement." But is this really so? It

would hardly be true of the relatively unorganized emotion, however it might be with hatred as a relatively organized and deep-rooted sentiment. Here the dispute tends to become verbal; and, in any case, more delicate tests would be required than could possibly be made in an off-hand judgment. However, these articles are among the best that have been written in criticism of current theories and should be read by all who are interested in the subject.

In the third article Dr. Irons analyses a number of specified feelings, with a view to discover the primary emotions, as distinguished on the one hand from their varieties and complications, and on the other from feelings which are not emotions at all. The conclusion reached is, that the primary emotions are satisfaction and dissatisfaction, anger and fear, ill-feeling and kindly feeling, repugnance, scorn, admiration. This result is novel both in its exclusions and its inclusions. It excludes, *e. g.*, not only 'self-feelings' like pride, humility and shame, and 'intellectual' feelings such as surprise, astonishment and wonder, but also æsthetic, moral and religious feelings, all which are regarded, some as not properly emotional at all, some as complex states into which one or more of the primary emotions named enters; and it includes, as specific emotions, the feelings of satisfaction and dissatisfaction, in which the author finds respectively the properly emotional elements in joy and grief. How far Dr. Irons departs from popular conception appears in the statement (p. 641) that in horror and terror the emotional element as such is overshadowed by its concomitants—organic sensations being among the concomitants—and in the denial (p. 645) that there is any emotional element whatever in the feeling of the sublime. These results seem necessitated by the author's conception of emotion as 'feeling-attitude' and as 'reaction,' these terms being used by him, as now appears, in a narrow and quite special sense.

Recherches expérimentales sur l'Excitation et la Dépression. G. Dumas. Revue Philosophique, XLIII., 623–634. June, 1897.

Finally an attempt to determine the relation of emotion to bodily conditions by counting the blood-corpuscles! It is well known that, in connection with osmosis of the serum, the number of the corpuscles in a given quantity of blood, in any individual, varies considerably. It occurred to Dr. Dumas to study this variation in states of excitement and depression. He experimented on two morbid subjects, who exhibited these states in regular alternation, and on himself. Sixty daily observations of the first subject, a woman whose period of excite-

ment lasted, on an average, 12 days, followed by an average period of depression of 16 days, showed a rapid increase in the number of globules in the transition to the state of depression, and a rapid decrease in their number in the transition to the state of excitement. The periods between the crises were marked by gradual variations of such a sort that the approaching change could be predicted. The author interprets these facts as follows: The rapid increase at the beginning of the periods of depression corresponds to the vaso-constriction which accompanies and determines the depression; the rapid diminution which announces the period of excitement, corresponds to the vaso-dilation which determines the excitement. That the organic change as revealed by the microscope anticipates a mental change unforeseen by the subject, proves the thoroughly mechanical determination of the latter; "the phenomenon of emotion, the subjectivity of which metaphysical psychologists have been pleased to exaggerate, is here objectively given before it exists subjectively at all." On the second subject, a man whose states alternated regularly from day to day, ten observations showed a regular rise and fall in the number of the corpuscles corresponding respectively to the states of depression and of excitement. A comparison of five observations of the globules during morning depression with an equal number of observations of them during evening excitement in the author's own person exhibited the same law. Dr. Dumas concludes, among other things, that the counting of the blood-corpuscles is quite an exact method of physiological psychology, one which can inform us of the imminence of one emotional state and of the approaching end of another and *perhaps even of the intensity of an emotion!*

H. N. GARDINER.

SMITH COLLEGE.

Beauty and Ugliness. VERNON LEE and C. ANSTRUTHER-THOMPSON. The Contemporary Review, Vol. LXXII., pp. 544–569.

In this article we have a new theory of the perception of form, and of our æsthetic pleasure in formal beauty. The explanation is based upon the James-Lange theory of emotion as the conscious correlate of certain physical changes, ordinarily considered its expression. The purpose of the authors is to show that the perception of form takes place by means of organic processes connected with respiration and equilibrium, and that those forms are æsthetic which further these processes. The æsthetic instinct is thus that which presides over

form perception, and, far from being useless, is connected with functions of vital importance.

The evidence for this theory is of a kind which is usually overlooked, since we are apt to pass over the subjective feelings accompanying perception and attend solely to the content perceived. Yet experiment shows that changes in breathing accompany the movements of the eyes in form perception. A bilateral object calls both eyes and both lungs equally into play. The upward and outward sweep of the eyes is accompanied by inhalation, deep in proportion to the height of the object; and similarly, expiration follows a downward and inward curve, or the attainment of a resting point. Changes in equilibrium also follow the movements of the eyes. The balance shifts with the perception of the form and, as it were, imitates it.

To determine the cause of the pleasure or pain in the perception of forms, a series of experiments was made in the perception of objects ranging from complete formlessness to perfect form. (*a*) Looking at a blank wall, the space in front seems to come forward. We feel as if our profiles were flattened and we had lost our identity. The breathing is weak, and holding the breath produces no change in the perception. (*b*) In looking at a confusion of objects, the eye focusses and there is no sense of one's profile being flattened. The breath is steadier and more under control, but holding it produces no change. (*c*) In the perception of a triangle the thoracic movements come into play and seem to make three little pinches at the three corners. There is a sense of resistance all around. In looking at a jar, however, our sensations become composed, balanced, and co-related in their diversity. Its equal sides bring both lungs into use, and its curves are followed by the breath and balance. We feel a totality in our movements corresponding to the harmonious whole of the jar. The æsthetic form is thus that which calls out complete and harmonizing activities of breathing and balance, activities which are of vital importance for the welfare of the individual. The æsthetic instinct is no mere play impulse, but exercises a selective and controlling power over all the constructive and expressive energies of mankind, and completely dominates practical life. The various fine arts are arrangements, spontaneously and unconsciously evolved, for obtaining the maximum of agreeable activity on the part of our perception of form. From the simplest pattern up to the most complicated work of art we find the same result, the movements of eye and breath are made rhythmical and deliberate, and reënforced by the feelings of balance.

NORMAN WILDE.

COLUMBIA UNIVERSITY.

British Moralists. L. A. SELBY-BIGGE. Vols. I., II. Oxford, Clarendon Press. 1897.

The compiler of these volumes has very successfully grouped together characteristic selections from the British ethical writers, principally of the eighteenth century. This work will prove of special value in classes where access to the original productions is necessarily impossible. Here in a small compass we have the essential features of a century's ethical thought. The selections are from Shaftesbury, Hutcheson, Butler, Adam Smith, Bentham, Clarke, Balguy, Price. The first volume is devoted to representatives of the sentimental school, and the second to those of the intellectual school of ethics. The second volume contains a long appendix, indeed over one-half of the volume, containing selections which might have been properly incorporated in the main text. There is also an introduction upon the general subject of eighteenth century ethics. It is an admirable essay and presents a point of view which will prove helpful to the reader as he takes up the study of the various authors of that period. There are two general characteristics of these writings which naturally recommend themselves to those who are imbued with the modern spirit of investigation. The first is that these writers built upon a psychological basis, and in the second place, their method was largely inductive. Their endeavor was to analyze the consciousness of the plain man, interpreting its content in a common-sense way, and collecting facts over wide fields in order to ground their theories upon firm foundations. The reading of the introductory essay and the following selections can not fail to leave the impression that the eighteenth century moralists manifested a spirit which may well be imitated by the present day students of philosophy, namely, of seeking an answer to the riddles of existence in the facts of common consciousness, and possessing as well the one faculty of stating plain truths in plain words. To receive such an impression is alone well worth the careful reading of these volumes.

JOHN GRIER HIBBEN.

PRINCETON UNIVERSITY.

Zur Psychologie der logischen Grundthatsachen. HEINRICH GOMPERZ. Leipzig, Deuticke, 1897. Pp. 103.

Dr. Gomperz to the contrary, the psychology of the fundamental facts of logic can not in these days rest on only a formal synthesis of Nominalism, Realism, and Conceptualism. A radical change of standpoint is necessary. "*Die Abhängigkeit des discursiven oder*

begrifflichen Denkens von der Sprache" is indeed a prevailing principle of thought, at which Dr. Gomperz may have arrived independently, and upon which others in earlier, as well as in more recent times, may be in accord with him (see the *Vorbemerkung*), but the value of the principle depends altogether upon the intent of the term *Sprache*. In this laborious investigation, however, we read (p. 23): "*Erstlich wird heute wohl fast allgemein zugegeben dass die Sprache ursprunglich eine spontane Reaction sei, und seitdem wir nicht an den Namengeber des Altenthums glauben, bleibt uns kaum eine andere Wahl.* * * * * *Zweitens aber darf man behaupten, dass die Sprache als solche in erster Linie eine Zeichengebung, also ein Verständigungsmittel ist.*" Here, then, we see what *Sprache* means to the author, whose work we are now considering; and it is hard not to apply even to his view, which he does in truth share with many others, the same abusive term that he, quoting Romanes, has cast (p. 24) at the conclusions of such as Hobbes and Max Müller, for it is certainly a '*transparente Absurdität*' not only with Hobbes or Müller to think of language as a body of arbitrary symbols but also with Dr. Gomperz to imagine language as '*ursprunglich eine Spontane Reaction.*' Spontaneity, a term even now too commonly used with reference to origins, does not bring any real freedom from belief in the *Namengeber*, so that, as was hinted above, the author now before us has not escaped the old-time standpoint, the dualism, which whether Realistic or Nominalistic or Conceptualistic makes name and idea two things instead of one. Spontaneous generation, of life or of language or of anything you like, may be a modern doctrine—although the early Greeks, not to mention others, were not without it, but it is not a modern standpoint; it may be a protest, a reaction, but like mere reactions in general it is an escape only from the letter, not from the spirit of its opponent; in short, positively, it is supernaturalistic or creationalistic. Nor is the second assertion, that language, as such, is "*in erster Linie eine Zeichengebung, also ein Verständigungsmittel*," any more liberative, since, as a matter of course, the same dualism underlies it.

Many of Dr. Gomperz's statements have possibilities in them. For example (p. 26): *Unter einem Worte wurden alle jene Einzelvorstellungen zusammengefasst welche zur Zeit der Sprachentstehung gleiche Reactionen hervorzurufen geeignet waren.* Here also, however, the writer's notion that reactions are 'spontaneous,' and so arbitrary, is hopelessly corrupting. And for further indications of the same corruption see p. 14, where, as in other places also, the absence

of certain words in savage languages—assumed on the authority of men who have been rather material-collectors than thinkers—is used as real evidence; or pp. 20 sq., where the word and the concept, although not without some sense of responsibility to the existing objections, are still treated as *elements* of language and thought respectively; or pp. 29 sq., where the analysis, however subtle, is wholly destructive of the reality under investigation, as with all dissection or vivisection; or pp. 34 sq., where the dualistic standpoint of Nominalism and its sympathetic opponents is boldly avowed; or pp. 19, 41, 75 and 101, where tell-tale summaries appear; or see, finally, nearly every page, at the bottom, where minute bibliographical references, seldom less than two and often as many as four or five in number, are effectively arrayed.

What, then, can be said of this book? Briefly, that Dr. Gomperz, although complying with existing ideals of work and loyal to still current doctrines or standpoints, is not, when all is said, a contemporary *thinker*.

ALFRED H. LLOYD.

UNIVERSITY OF MICHIGAN.

NEW BOOKS.

The Evolution of the Idea of God: an Enquiry into the Origins of Religion. GRANT ALLEN. New York, Holt. 1897. Pp. ix + 447. $3.00.

La Certitude Logique. G. MILHAUD. 2d edition. Paris, Alcan. 1898. Pp. viii + 204. Fr. 2.50.

Problems d'Esthétique et de Morale. C. R. C. HERCHENROTH. Paris, Alcan. 1898. Pp. 164. Fr. 2.50.

Etudes d'Histoire de la Philosophie. É. BOUTROUX. Paris, Alcan. 1897. Pp. 444. Fr. 7.50.

La Personne Humaine. L'Abbé C. PIOT. Paris, Alcan. 1897. Pp. 401. Fr. 7.50.

La Vie mode de Movement. E. PRÉAUBERT. Paris, Alcan. 1897. Pp. 310. Fr. 5.00.

Raumaesthetik und geometrisch-optische Taüschung. TH. LIPPS. Schriften der Gesell. für Psycholog. Forschung (II. Sammlung heft 9–10). Leipzig, Barth. 1897. Pp. viii + 424. M. 12.

Die Entwickelung des Geistes beim Kinde und bei der Rasse. (*Methoden und Verfahren.*) J. MARK BALDWIN. Uebersetz von A. E. ORTMANN. Nebst Vorwort von TH. ZIEHEN. Berlin, Reuther u. Reichard. 1898. Pp. xiv + 470. M. 12.

Die räumlichen Wahrnehmungen des Tastsinns. V. HENRI. Berlin, Reuther u. Reichard. 1898. Pp. 228. M. 7.50.

Le Développement mental chez l'Enfant et dans la Race. J. MARK BALDWIN: Traduit par É. NOURRY, avec une Préface de L. MARILLIER. Paris, Alcan. 1897. Pp. xiv+464. Fr. 7.50.

Ethics. WILHELM WUNDT. Translated by JULIA GULLIVER, MARGARET FLOY WASHBURN and EDWARD BRADFORD TITCHENER. New York, The Macmillan Company; London, Swan Sonnenschein & Co., L't'd. 1897. Vol. I., xii+339, $2.25; Vol. II., pp. viii+196. $1.75.

Psychologie als Erfahrungswissenschaft. HANS CORNELIUS. Leipzig, B. G. Teubner. 1897. Pp. xv+445. M. 10.

Magic, Stage Illusions and Scientific Diversions. Compiled and edited by ALBERT A. HOPKINS. New York, Munn & Co. 1897. Pp. xii+556. $2.50.

Memory and its Cultivation. F. W. EDRIDGE-GREEN. New York, D. Appleton & Co. 1897. Pp. 311.

Elements of the Science of Religion: Part I. Morphological. C. P. TIELE. New York, Imported by Charles Scribner's Sons. 1897. Pp. viii+312.

NOTES.

THE death is announced of Professor Henry Calderwood, since 1868 professor of moral philosophy in the University of Edinburgh, on November 20th, at the age of sixty-seven years. He was the author of numerous publications on education, psychology and philosophy, including a work on the 'Relations of Mind and Brain' (1879), which was one of the first systematic treatises on physiological psychology. We regret also to record the deaths of two eminent physiologists, Professor Holmgren, of Upsala and Professor Haidenhain of Breslau, both of whom had made contributions of importance to psychology, in addition to such as were strictly physiological in character. Especially well known are Holmgren's studies on color-blindness, and Haidenhain's on hypnotism.

Of recent American appointments we note that James E. Russell, A.B. (Cornell), Ph.D. (Leipzig), has been appointed professor of psychology and educational methods in the Teachers' College, New York, while Norman Wilde, A.B., Ph.D. (Columbia), is giving instruction in psychology in the Teachers' College in addition to his work in Columbia University. Dr. Arthur Allin succeeds Dr. Russel as professor of psychology and pedagogy in the University of Colorado. Dr. Warner Fite, assistant professor of philosophy in Williams College, has been appointed to a docentship in philosophy in the University of Chicago, and Mr. A. F. Buck and Miss Jane Downey have been appointed assistants in the psychological laboratory. The chair of philosophy and the chaplaincy of Lehigh University have been filled by the election of the Rev. Langdon C. Stewardson, rector of St. Mark's Episcopal Church, Worcester, Mass. At Oberlin College S. F. McLennan, A.B. (Toronto), Ph.D. (Chicago), has been made associate professor of psychology and pedagogy.

Abroad, Mr. Johnson, of King's College, Cambridge, has been appointed university lecturer on moral science, and Mr. J. E. McTaggart lecturer on the same subject in Trinity College, Cambridge. Dr. Max von Frey, of Leipzig, has been called to the chair of physiology at the University of Zurich. Dr. Max Dessoir has been promoted to an associate professorship of psychology in the University of Berlin. Dr. Forel, professor of psychiatry in the University of Zurich and Director of the State Asylum for the Insane, has resigned these offices, owing, it is said, to the attacks made upon him by the press for the part he has taken in combating the use of alcohol.

A Croom Robertson fellowship with an endowment of £8,000 has been created in the University of Aberdeen, with which Robertson was connected before being called to the Grote chair of philosophy of mind and logic in University College, London. The fellowship will be awarded alternately in philosophy and in the classics.

The Philadelphia Normal School has this year established a psychological laboratory.

Two new museums will be opened at St. Petersburg in connection with the Army Medical Academy, one for psychiatry and one for neurology. The former will include objects illustrating the arrangements of asylums and the pathology of insanity, while the latter will illustrate the anatomy of the nervous system.

A MICROTOME for making sections of the entire human brain is being constructed by Messrs. Bausch and Lomb on the pattern of Professor Minot's 'automatic precision microtome.'

A SCHOOL of ethics and social philosophy has been formed in London, with a general committee including the Master of Balliol, Mr. Leslie Stephen, Mr. R. B. Haldane, Mr. W. L. Courtney, Professor Sully and Mrs. Bryant. The aim of the new school is to approach social questions from the side of psychology and ethics. Lectures have been promised by Professor Henry Sidgwick, Mr. Sidney Webb Mr. F. W. H. Myers, Mr. Bosanquet, Professor Lewis Campbell and others.

THE *Philosophical Review*, edited by President Schurman and Professors J. E. Creighton and James Seth, of Cornell University, and published since its foundation by Ginn & Co., will next year be published by The Macmillan Company.

A TRANSLATION of the 'Ethics' of Professor Paulsen, of Berlin, is announced by Messrs. Charles Scribner's Sons, and one of Groos's 'Spiele der Thiere' by the Appletons.

THE *Atlantic Monthly* promises for next year a group of articles on modern psychology and its contributions to education.

THE Minnesota Child Study Association, organized in 1895, has published a handbook of sixty pages, which may be secured for thirty cents from the secretary of the association, Mr. E. A. Kirkpatrick, Winona, Minn. The pamphlet contains a number of suggestions and syllabi for the study of children, which will prove useful, more especially in view of the lack of a systematic treatise on the subject.

IN addition to volumes of the Contemporary Science Series (Mr. Walter Scott, London; imported by Messrs. Charles Scribner's Sons, New York) requiring special notice, we have received two volumes—one a translation and one a new edition, to which attention may be called. Professor Ribot's *Psychologie des sentiments*, of which we gave a detailed review at the time of its publication, has been translated into English, thus much enlarging the audience of the acknowledged leader of French psychology. Dr. Moll's *Hypnotism* has been issued in a fourth edition, being apparently more in demand than any other volume of the series.

J. McK. C.

L'Intermédiare des biologistes, the plans for which we announced in our last issue, has now begun publication, the first number being dated November 5th. It will be published twice a month, and the

present number contains twenty-four pages. In addition to an introduction we find a short article by M. Marey, advocating the plan brought forward by him at the recent meeting of the French Association in favor of an international control of physiological instruments; forty-six questions to which replies are requested; the contents of the special journals of zoölogy, botany, physiology and psychology, and short descriptions of three instruments. The journal throughout shows the psychological interests of Professor Binet and his associates in the editorship. In the introduction it is stated that a complete bibliography of the literature, so far as it is contained in other journals, will be given, but we think that the magnitude of this undertaking is scarcely realized. The series of questions is a feature that has, perhaps, not been undertaken in a scientific journal and may prove of value. Among thirty-four 'collaborateurs' there are two Americans, Professors Minot and Baldwin, and one Englishman, Dr. Sherrington. The price of subscription for America is 12 fr., to be sent to the publishers, Schleicher Frères, Paris.

J. McK. C.

THE Open Court Publishing Company, Chicago, propose issuing a series of life-size portraits of eminent philosophers and men of science, should the advance subscriptions warrant the undertaking. They have prepared a preliminary list of sixty-two philosophers and psychologists and offer this set for $7.50. We trust that this very reasonable price, and the value of the work already done in this direction by the Open Court Publishing Company, will secure a sufficient number of subscriptions to lead them to publish the series, and to follow it, as they suggest, with series of mathematicians, physicists, biologists, etc. It is perhaps worthy of note, as indicating tendencies of scientific development, that, of the thirty-six philosophers given in the series drawn up by the Open Court Publishing Company, all are dead except one, whereas a large proportion of the psychologists are now living. The list of psychologists is as follows: Cabanis, Maine de Biran, Beneke, G. E. Müller, E. H. Weber, Fechner, Helmholtz, Hering, Aubert, Mach, Stumpf, Munk, Exner, Wernicke, Steinthal, Brentano, Paul Janet, Ribot, Taine, Fouillée, Binet, Bain, Romanes, C. Lloyd Morgan, Bastian, James. The omission of Wundt's name from the list was through inadvertence. In the English names biology is emphasized at the expense of psychology. The names of Ladd and Hall should be added to give America its fair representation.

J. McK. C.

We have received a copy of 'Nirvana, a Story of Buddhist Philosophy' (Chicago, Open Court Co., 1896, pp. 46), by Dr. Paul Carus, the versatile and accomplished editor of the *Monist*. 'Nirvana' is printed and illustrated in Japan, showing both the style and the detail of Japanese book making, and forms one of the most elegant publications of the holiday season. The story expounds very clearly the Buddhist doctrine of 'Nirvana.'

J. M. B.

All communications for the editor of The Psychological Review, together with books, reprints, etc., intended for review, should be sent, during the year ending November 1, 1898, to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.